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DESIGN.

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IN the Introduction to his just published edition of Hume's *Dialogues concerning Natural Religion*,¹ Professor Kemp Smith remarks (p. 38) that the *Dialogues* have a "unique place in philosophical literature. They are unique in two respects. As Leslie Stephen has pointed out, they were the first work in our literature to subject the argument from design to a passionless and searching criticism. And secondly, Hume's destructive criticism of the argument—allowing for the limitations under which it was formulated—was final and complete. For a couple of generations, theologians, especially in Britain, may have continued on the old lines, as if the *Dialogues* had never been written. But in the altered outlook of present-day theology, these older ways of argument have in large measure ceased to be approved, and Hume's indictment of them is now but seldom challenged." Kemp Smith admits that the argument from design, in the form in which Hume² criticised it, not only "persisted into the nineteenth century in the writings of Paley" and others, but "survives in popular and semi-popular forms to the present day" (p. 36). He appears to think, then, that these uncritical conceptions, like those of popularisers of the Jeans type, carry no great weight not merely among philosophers but even among the more serious theologians. This is a view which, while I have no great acquaintance with present-day theology, I should be very much inclined to question. For, apart from the mere ignoring of Hume, there is also the tendency to substitute for the criticised doctrine other doctrines which are open to the very same type of criticism. And both these reactions are facilitated by Hume's "scepticism", i.e., by his failure to work out a *logic*.

Arguments like that of design, in fact, cannot be thoroughly gone into until attention is directed not merely to

¹ Clarendon Press.

² I share the view, and shall assume it in this article, that, allowing for dialogue form and the social conditions which, as Kemp Smith shows, prevented complete candour, Philo presents Hume's own position. Kemp Smith's demonstration, it seems to me, should put any other interpretation of the *Dialogues* finally out of court.

the illogicality of the proofs offered but to the illogicality of what they are supposed to prove. And, whatever suggestions Hume may make towards a thorough-going solution, he cannot arrive at it and force subsequent attention to it, because of the defects in his philosophical outlook—because, like the other “English empiricists”, he was rationalistically concerned with “ideas” (that whose nature it is to be perceived) and not with propositions (what is the case). So long as any admission of “natures” is made, dualism is inevitable; and even if the term “design” is abandoned, some comparable and equally confused way in which the nature may “express” itself must be retained. Hence, while Hume could point empirically to many of the difficulties of the theory of design, not only could he not prevent it from raising its head again, but his own rationalist preconceptions were an influence towards its doing so. Similarly, it may be remarked in passing, it is because of its rationalism, its retention of “principles” and the like, that the realistic movement set going by Moore and Russell has failed—failed, i.e., to work out a realist *philosophy*. To reap the full benefit of Hume’s work, then—and the same applies to the work of the later movement—it is necessary to follow up the questions raised, to cut away rationalist conceptions and so to arrive at a logical position.

That progress has been made in this direction by philosophers (whatever may be said about theologians and pseudo-philosophical scientists) is due not merely to Hume’s work but to the fact that Kant, as Kemp Smith points out (p. 39), incorporated Hume’s main criticisms in his “Transcendental Dialectic”. But, whatever stimulus Kant may have given to logic, he remains a dualist, and theories which do not go beyond Kant’s position can be criticised in Philo’s manner, even if not with his precise arguments. One of the most important points made by Kant is that the physico-theological argument, as he calls it, is not an argument *a posteriori* or from experience, but has as much of an *a priori* or ontological character as any of the others, that it depends on the conception of that which is ultimate or establishes itself. It should be emphasised here that ontological arguments are not confined to proofs of the existence of God or of something described as *the Absolute*, but are the means of establishing all ultimates, even in nominally pluralistic theories. It is really in the ontological fashion that the Pythagoreans set up their units or Socrates his forms or that anyone sets up *that which is by its own nature*. It seems indeed to be shown by the Eleatic criticism of the Pythagoreans, and similarly by Spinoza’s criticism of Descartes, that there

cannot be more than one ultimate, but the more important question is whether there can be one at all. And until it has been shown that there cannot, until the very conception of "ultimacy" has been rejected, until it has been demonstrated not merely that certain arguments are unsound but that their supposed conclusions are untenable, the position has not been worked out and the same types of error recur. Thus not only can we say that Kant argues ontologically in his theory of the good will, i.e., that which establishes itself by willing itself (just as in the conception of consciousness, by theorists from Descartes onwards, as that which establishes itself by thinking itself); we must also note that the very conception of the "thing-in-itself" involves an ontological argument—for what is an argument but a recognition of dependence?—and that the only solution is not a problematical recognition but the rejection of *that which is in itself*.

This is connected with the inseparability of cosmological and ontological arguments. Kant shows that the cosmological argument depends on the ontological, that that on which all other things depend can only be that which establishes itself. But, equally, that which establishes itself has to be taken as that on which other things depend, because in its very conception there is the distinction between its character of establishing and its character of being established; and, while the latter now appears, like a created cosmos, as having "dependent existence", the same problem as before breaks out in regard to the "self-subsistence" or self-supporting character of the former, the same distinction has to be made between its supporting and its being supported, and so on indefinitely. There is likewise no logical division between cosmological and physico-theological or teleological arguments, because in each case we have the dualism of ways of being, that which has its being in supporting and that which has its being in being supported. The only way to escape from the vicious circle, in which dualism collapses into monism and monism explodes into dualism, is to adopt a pluralistic position in which variously characterised and related things are recognised as existing in the same way (spatio-temporally)—a single logic of existence replacing conceptions of "self-subsistence", "relative existence" and any other flights of rationalistic fancy.

It is worth noting that all theories of higher and lower realities are stated in terms of the common reality we all know—and, indeed, can be stated in no other way. Thus, as Berkeley points out in criticism of Locke, we are all acquainted with the way in which one thing supports another, we know the empirical relation of supporting, but we are not

acquainted with the way in which "matter", in the Lockian theory, supports accidents. Locke had to use an ordinary term, at the same time suggesting that it was to be understood in an extraordinary sense; but he could not even begin to tell us what that sense is, how the "supporting" of accidents by matter differs from the supporting that we know. Thus the common relation gives us no help in the understanding of the metaphysical one, but the use of the common word tends to confuse our minds and make us *imagine* that we have understood something. The force of Berkeley's criticism here is not weakened by the fact that his position is open to the same objections, that he utilised our acquaintance with dependence as an actual relation between actual and distinct things, in order to make it apparently intelligible to us that certain things have "dependent existence". In so doing, in separating the active from the passive, the effective from the effected, he takes up a position logically indistinguishable from that of Cleanthes (and, of course, closely resembling it in detail; Berkeley, we may say, was a notable participant in that movement in eighteenth century thought which Cleanthes represents in the *Dialogues*), but in his criticism of Locke he did some of the work of Philo.

What Philo does, in fact, is to lay bare just such empirical material as the rationalists use in their arguments, and to show that it will not bear the metaphysical doctrines erected on it. All that his criticism lacks is development in the direction of a thorough-going empiricist logic, which would show that his declension to scepticism is uncalled for. For the sharpening of the logical issues, then, we may take as our point of departure the famous passage (Part II) in which Cleanthes sets forth the main theme of the discussion. "Look round the world: Contemplate the whole and every part of it: You will find it to be nothing but one great machine, subdivided into an infinite number of lesser machines, which again admit of subdivisions, to a degree beyond what human senses and faculties can trace and explain. All these various machines, and even their most minute parts, are adjusted to each other with an accuracy, which ravishes into admiration all men, who have ever contemplated them. The curious adapting of means to ends, throughout all nature, resembles exactly, though it much exceeds, the productions of human contrivance; of human design, thought, wisdom, and intelligence. Since therefore the effects resemble each other, we are led to infer, by all the rules of analogy, that the causes also resemble; and that the Author of nature is somewhat similar to the mind of man; though possessed of much larger faculties, proportioned to the grandeur of the work,

which he has executed. By this argument *a posteriori*, and by this argument alone, do we prove at once the existence of a Deity, and his similarity to human mind and intelligence."

The weakness of analogy, the question-begging character of the reference to *the* Author of nature, are obvious points of criticism here. But a more important line of attack is that which proceeds from the fatal admission of Cleanthes that there is nothing which does not bear the same marks as are borne by the productions of human contrivance, i.e., that what he regards as marks of contrivance are conditions of existence. If we could distinguish the contrived from the uncontrived, then we might be able to discover certain marks of the former; but if everything bears certain marks, then, naturally, human contrivances will do so, but this will be no argument in support of the view that they are marks of *contrivance*. As far as human operations are concerned, the materials worked upon, the things not humanly contrived, will bear the marks in question just as much as the things produced or humanly contrived. Accordingly, the alleged *a posteriori* argument from the special mode of operation of human contrivance, conceived as the *introduction* of marks of design, disappears, and we are left with an *a priori* argument, of the cosmological type, to the effect that everything in nature, whether it is worked upon by human minds or not, is necessarily dependent on some creator or source or has a *subordinate* form of existence. Such a position is involved, indeed, in this very use of the term "nature" (generally, in our day, with a capital). Now, as we have seen, even an *a priori* position has to make use of empirical material; we could never entertain the conception of dependence as a peculiar form of existence (i.e., as a qualification of the *copula*), unless we were acquainted with it as a character of actual situations—unless we had had experience of the occasioning of one thing by another, whether the latter is a mind or not. But there is nothing in this experience to warrant the conception of dependent existence; on the contrary, we are concerned with whether or not the one thing is *actually* dependent on the other—and not with whether it is *dependently* dependent, and so on indefinitely, as we should have to assert if we took it to have this peculiar mode of being.

Logically, then, even if there are marks of design, this is only a matter of a particular relation between different sorts of particular things, which are all alike "actual" or, as I should prefer to put it, spatio-temporal. But Cleanthes is quite correct in taking the adjustment he refers to as characteristic of things in general, i.e., anything we like to

take has characteristic ways of working, one phase of which leads on to another, and acts differently in different situations. Accordingly, since adjustment is found alike in the things we ordinarily call machines and in those we do not, there is no ground for tracing the adjustment of the former to the fact of their being machines and thence concluding that the latter also are machines produced by some non-human contriver; just as there is no ground for Berkeley's argument that those of our "ideas" which we do not control must be controlled by some other mind, but a mere assumption, in spite of appearances to the contrary, that control or efficacy is always mental, so that working on mind must be working by mind. There is then, no special "working" character of mind and no special "wrought" character of machines or other things. According to the admissions of Cleanthes—and if he does not make them, his argument cannot proceed—human contrivance consists not in making things work together, since they already do so, but in making things work together in a certain way, i.e., in making certain "workings" or activities, which simply means *making certain things*.

Human contrivance also involves the working of things on minds, but that is a matter to which I shall return. Meanwhile, it may be of some interest to consider another incidental point. If human contrivance consists of making things which worked together in a certain way work together in a different way, then we have human interference with God's arrangements; in other words, Cleanthes, in starting from human contrivers, has no argument for a single contriver but must admit a multiplicity of competing contrivers. The alternative is to take human beings also as contrivances of the original contriver, and not as contrivers at all. A similar dilemma faces Berkeley in his theory of "the conduct of life" and of our knowledge of other finite minds. Unless the conduct of life consists merely in imagining what we like, without the slightest effect on the sensations that are forced on our minds, Berkeley must admit that we can operate on God's ideas, i.e., that we can give him new ideas just as he can give us. And, again, unless other minds can alter God's ideas, we cannot have the slightest reason for taking any particular sensation as indicative of the operation of another mind, but only as coming direct from God. Finally, as in the case of Cleanthes, Berkeley either has to admit that we are simply some of God's ideas and have no "agency" whatever, or has to recognise a thoroughgoing interaction and abandon his doctrine of the "active" over against the "passive".

To return to the main argument, human contrivance means that human beings, by "thinking" and other operations,

produce certain things, and it may well be that *only* human beings produce things of certain sorts. In that case, if we see such a thing, we can infer that a human being made it; but the inference depends not on our finding in the thing a peculiarly designed or contrived character, but, as Philo points out, on our knowledge of the fact that things of that kind *are* made by men. We have all seen men going through a series of operations culminating in the existence of a house, and we have never seen a house coming into existence in any other way. It is on this account that, when we come across a house which we did not see being built, we conclude that somebody built it, and not because we see a contrived character in it. But, if Cleanthes were right in finding contrivance in everything, then we should be just as ready to conclude that a house or a ship which we did not see constructed had been made by the contriver of things in general and that no man had a hand in its construction, as to draw the conclusion that we do. Moreover, if Cleanthes were right, he would have no need of any analogy; he could argue straight to a designer from the designed character of anything he liked to consider, without having to refer to human performances. The fact is that there is no designed or contrived character, that contrivance is a relation between different things and not a character of either by itself. Knowing such relations (e.g., all houses are made by men) we can draw inferences—we can infer the human contrivance of some things, the mouse or bird contrivance of other things, and so on—but never contrivance by *the* contriver, and always on the basis of experience of things of the sort A contriving things of the sort B.

The fact that Philo does not enforce the *logical* issues, does not insist, e.g., that “the adjustment of means to ends” signifies merely that when something happens to a thing it does something else, accounts for his being able at the conclusion of the argument to put forward the modified, and, as he admits, useless, assertion “that the cause or causes of the order in the universe probably bear some remote analogy to human intelligence”. Nevertheless, his suggestion that what is really required is revelation may quite well not be ironical, but, taken in conjunction with his earlier argument, may rather indicate that Hume had some conception of the weakness of relativism, the impossibility of characterising a thing by its relations or of taking the character of a thing *in itself* as testifying to the character of a related thing. This, indeed, is the very point that Hume made in regard to causality in the *Treatise*; but only the abandonment of his rationalistic theories of “ideas”, “relations of ideas”, and, still more

important, spatial and temporal *units*, would have enabled him to bring these questions to a decisive issue.

Cleanthes, at any rate, does not and cannot explain why what he takes to be provable is not also observable, just as, when we see a house and infer that men built it, we also believe that someone saw them build it. He wishes to argue to the existence of something with which we are not acquainted and whose modes of operation we therefore do not know, on the ground that these (unknown) modes of operation are the only thing that will account for the existence of something with which we are acquainted; and a good deal of Philo's "scepticism" is just the bringing out of the sceptical character of this position. The particular contention that a greater contrivance implies a greater contriver is one which, as Philo suggests, we are not in any way bound to accept; for if by a greater contriver we merely mean one who makes a greater contrivance, we have not indicated any characteristic of his which we can call his greatness and which can distinguish him from lesser contrivers—and if we try to specify such a characteristic, if, e.g., we make it a matter of physical size, then, while it is still a question of experienced connections, of *finding* a bigger thing making a bigger thing, we must admit many contrary cases of a physically smaller cause having a physically more extensive effect. Only its vagueness can protect such a contention, which is still an attempt to find in the effect a positive characterisation of the cause.

Equal difficulty is found in the attempt to characterise the cause negatively from the effect. Cleanthes argues—all machines are designed by minds (which we have seen to be a mere assumption, if all that is meant by a machine is something which has characteristic modes of operation under various conditions); the universe is a machine not designed by a human mind; therefore, the universe is designed by a non-human mind (and, as above, since the universe is greater than any human contrivance, this non-human mind must be greater than any human mind). Now, leaving aside for the present the questions arising in connection with the conception of "the universe", we may object to the above argument that, unless we had independent evidence of the existence of a non-human mind, or if human minds were the only minds we knew, the acceptance of the second premise would lead us to cast doubt on the first. In other words, we should require to have some acquaintance with non-human minds before this argument would produce conviction, and consequently we cannot take it as a proof of the existence of a non-human mind or a means whereby we could be led for the first time to suppose a non-human mind to exist.

This criticism has a certain similarity to Alexander's criticism of the view that we argue to the existence of minds other than our own by analogy, i.e., that, since we know that certain bodily processes are associated with certain of our own mental processes, then, when we find such bodily processes not associated with our own mental processes, we assume that they are associated with some other mental processes and thus recognise the existence of other minds. Alexander's contention is that such bodily behaviour could not give us the conception of "another mind", and that, if we were not independently acquainted with other minds, we should simply take it that the bodily behaviour in question was not associated with any mind. Apart, however, from the special difficulties in which Alexander is involved by his conceptions of our "assurance" of other minds and "enjoyment" of our own, it may be pointed out that if we *are* acquainted with our own minds, then we may, as Moore puts it, reasonably suppose a similar mind to exist in similar observed conditions. So that, even if inference is not the only or the regular way in which we learn of the existence of other minds, it may be (and is) one of the ways. But it cannot be the way in which we learn of the existence of a special type of mind, with which we are not otherwise acquainted. Or, if the last statement is one which must be received with caution, at least we do not so learn of a type of mind (or of anything else) with which we *cannot* be otherwise acquainted.

The caution referred to is enjoined by the following considerations: we may believe that things of the sort A come into existence by the contrivance of minds or mental processes of the sort a, B by b, C by c, and so forth. Then if we come across a thing of the sort ABC, we are thereby led to suppose the existence of a mental process of the sort abc, even if we have no direct acquaintance with such a process. There would still, of course, be the possibility of our being mistaken about some of our premises, and we might retain a lingering doubt about the existence of the mentality in question until we had observed it; but at least we were led to conceive it by that evidence. Even so, we think of its contrivance of ABC as a particular contrivance going on at a particular time, just as we found the contrivances of A, of B and of C to be in the first place. It is another matter entirely when it is a question of Contrivance as a whole; in that case there is not even the remotest analogy with human contrivance, there is no reason for calling the assumed "higher" operation contrivance at all.

As we saw, if we take positive account of human contrivance, we find that it is a relation between human minds

and other things, and that both exist in the same way, not that one "exists contrivingly" and the other "exists contrivedly". The question is then just to find what things human beings do contrive, and this raises no question of any contrivance of other things by other beings. Equally, there is no question of "marks of contrivance". But if, like Cleanthes, we take as marks of contrivance working in certain regular ways, having a certain harmony of parts, and so on, i.e., having a certain character or "constitution", then we must take the things we call contrivers as also contrived; the workings of the mind are of the same "contrived" character as the workings of any other thing. But to say in this way that all contrivers are contrived is not to argue for the existence of one great contriver who is not contrived; it is to argue against it. On the other hand, if we say that contrivers are not contrived, then we cannot take regular working, etc., as marks of contrivance, because they also have these marks, and so we have no ground for calling the ordinary "works of Nature" contrivances, and the argument of Cleanthes has not even a beginning.

There are, of course, additional difficulties in the way of the conception of an original contriver of the whole of "creation", but in the first place there is the same difficulty, viz., that if we are to say anything about this contriver at all, we must regard him as having characteristic ways of working, as being of the same logical order as his alleged "creation", as being a set of interacting situations—and, apart from the question of this set being itself situated or environed, we are already committed in those admissions to the rejection of an *origin* of things and to the treatment of the emergence of further situations in the same way as we treat developing situations now, viz., as exhibiting certain forms of action and interaction (though indeed this emergence and the implied pre-existence already constitute an environment). It seems of minor importance then to point out that there can be no contrivance of a "universe" or totality of things, because the contriver would have to be included in the totality of things; and if things constituted "one great machine", he would be part of the machine and not its maker. Alternatively, things would not constitute one great machine, but the least we could think about would be *the contriver contriving contrivances*—in which case, of course, there would be no *argument* to a contriver. Even that, however, does not provide an escape; but (as in the case of Berkeley's similar minimum of a *mind having ideas*) the different elements in this complex situation must be taken as all existing in the same way and none as having "higher" being than another. So that, as in the case of

human contrivance, we have a particular relation between particular things—or else we have nothing at all.

We see, then, that Cleanthes, in speaking of the contriver of the universe, is not seriously thinking of the latter as a universe or totality. But it is no more possible, even if we could separate things active from things passive, to think of a totality of created things (or "Creation", as it is called) than to think of a totality of things in general. Indeed it is a mere phrase, without any experience or serious argument to justify it, because if we do think of something we call Creation or The World, we can think of it only as certain things acting in certain ways—just as, if we thought of a creator, we could only think of him as acting in certain ways, and so could not make the logical distinction which the theory requires. But, speaking simply of the things Cleanthes calls machines, things to which we do not attribute any power of contriving, we do not find that they make up one great machine; we do not find any total situation or any way of working which is that of the whole. There is no object of which we can say, "That is the world with all its parts and characters adjusted to each other"; there is no observable situation of all things working together—for good or for anything else. On the contrary, in any observation, while we find adjustment or ways of working, they are always ways of working of particular things; and, even so, these are not all the ways of working of the things, apart from the influence of the other things with which they may come in contact. Thus there is no formula or law which will cover every action of any group of things, however large or small, that we like to take, and there is no question of one great machine or one total way of working.

The facts of complexity and interaction have a further bearing on the notion of a machine. As we have noted, contrivance is a relation between distinct and independent things—independent in the sense that, though a house, e.g., would not have existed but for the operations of men (and in that sense of dependence human beings are also dependent on inanimate things), it now does exist in the same way as they do, we can know it even though we know nothing about them, and we can interact with it in the same way as we interact with them. It is important to observe, then, that, apart from the fact that we can only operate on given materials, there is nothing which is a mere contrivance; anything that we contrive always has (without reference to *mistakes* we may make about it) characters which we did not anticipate. In the same way, of course, as against voluntarism, there are always characters

of our voluntary actions which we were not aware of when we willed them, so that they are not simply "our decisions". To say, then, that there is something more than we "contrived" about the things we have made is to recognise that they are independent things and to do away with any logical division between contrivers and contrived, and so with any conception of a totality of the contrived—or, for that matter, of a totality of any kind, as contrasted with the interaction of independent things. And to say that, while this may be the case with human contrivances, there is nothing about God's contrivances, or God's creation, of which he was not aware when he created it, is to destroy the analogy between God's contrivance and our contrivance, and to make the mere assumption that there could be a contriver who knew everything about his contrivances. There certainly could be nothing about his contrivances to prove that he did—though, if he did, he would know something, i.e., there would be two distinct terms to the relation of knowledge, and so there would still be independent existence of the contrived. But a more important point here is that, if God knew "all about" his contrivances, they would not be things at all but "natures", and the question of the contriving of things would be quite untouched.

Connected with this question is the further point raised by Philo that the order and contrivance put into the world by God implies a pre-existing order and contrivance of God's ideas. It is not a sound objection to Philo's argument here to say that the theory of ideas is false; because, if we admit that certain beings contrive a certain order or arrangement of things, we have to admit a certain order or arrangement in them, whatever it is an arrangement of. Thus, in order to *intend* to create an ordered world, God would have to have a certain arrangement of intentions or gestures towards what he was going to create; and, if these expressions are criticised as merely statements of relations, there is still implied a certain order of the related things, i.e., of the things which intend as well as of the things which are intended. Consequently, rejecting the theory of ideas (or simply saying that the only positive sense that can be given to "our ideas" is our demands), we can still say that Philo's line of argument is sound, that complexity and order in the contrived imply complexity and order in the contriver, and so, if order requires a contriver, the contriver must have a contriver. And it is not open to Cleanthès to say that, just as he need not, in knowing a causal relation, inquire into the cause of the cause, so he need not concern himself with how God came to be; because it is only by having a general contriver that we can have a general

contrivance or "Creation"—though, as we have seen, we cannot have it even then. The point also arises here as before, from the complexity of God's contriving, that any of his acts of contriving takes place among others, i.e., in an environment, and, even if we could limit that environment, that limited environment would be "the universe", so that the universe would not be created by any act of contriving. We note, further, that, while Philo has not succeeded in working out a logical position, he has raised some of the most serious logical questions.

It is in view of some of these difficulties, that Cleanthes goes back on his previous position and says, since it is on the analogy of men that he is arguing, that he is prepared to accept a theory of many gods operating as various men do, i.e., to treat "God" as finite, imperfect, many, just as men are. He would be quite satisfied, he says, so long as it was still admitted that there is design everywhere. But actually, on that view, he would be dropping his theory of the world as a total machine and substituting for it a theory of various single contrivances, which then would interact in a perfectly natural manner, without any question of all these interactions being contrived—though, as has been indicated, this would be the case even on the hypothesis of a single contriver. Thus there would *not* be design everywhere; and, incidentally, on his "pluralistic" theory, Cleanthes could give no reason for rejecting the view that anything we have not seen being made by men was nevertheless made by some man or body of men in the past ("Once a warrior, very angry", etc.); in other words, his position becomes manifest as one of pure guesswork or mythology. He has no escape, then, from the truly pluralistic position that even a designed thing is an independent thing, something which has its own existence and ways of acting—the alternative being that the designed has not its own ways of acting, since *designing* is the only way of acting, and hence that the designed does not exist, and consequently designing does not either.

The fact that we cannot think of a totality of things is brought out again when Demea, as the more consistent logician, refuses to make the admission that Cleanthes had made; for we find that Demea can give no account whatever of his all-causing deity, that his position is really a sceptical one, because he has to say that we can know only of the existence of God, or that we must postulate his existence, without knowing anything of his nature, which is in fact incomprehensible to us. This is so, of course, if he has a "nature". But that is only to say that doctrines of "natures"

or of the incomprehensible are quite otiose, that they give us no assistance whatever in knowing things—indeed they are hindrances. The point is that, if we know nothing of God's character, then we do not know what it is that is said to exist, and consequently Demea's position is a perfectly empty one. But so is the main position of Cleanthes, and it is no accident that, in the discussion of evil, he falls back on "incomprehensibility". It is indeed quite a common type of argument to try to make out that God's goodness is of a higher order than ours and that we cannot fully comprehend his purpose or we should recognise that all that men reckon evil has a place in the grand design, and meanwhile we have to take it as a matter of faith. This simply means that the postulation of a total and perfect design is to induce us to reject the knowledge that we actually possess. Descartes argues in a similar manner in comparing the perfection of deity with the imperfection of men and the errors they fall into on account of their limited intelligence. Now one definite objection to all such arguments concerning our limited intelligence is that it is our limited intelligence that is putting forward these arguments, and if there is any dubiety about our ability to understand these matters, then there is equal dubiety about the arguments themselves. But there is no reason whatever for scepticism or "suspense of judgment" so long as we have beliefs, i.e., so long as we find things having definite characters and acting in definite ways; and we always do.

It is curious that Philo should propose suspense of judgment (end of Part VIII) immediately after he has come nearest to presenting a position which would completely dispose of that of Cleanthes. "In all instances which we have ever seen, thought has no influence upon matter, except where that matter is so conjoined with it, as to have an equal reciprocal influence upon it. No animal can move immediately any thing but the members of its own body; and indeed, the equality of action and re-action seems to be an universal law of nature." Here, in spite of the rationalistic theory of equality (and the theory of ideas appears in the same passage), we have an approach to the recognition of interaction as a condition of existence, so that even a contriver is seen to be influenced by his material—just as Socrates, in his attempt to show that the mind "rules" the body, cannot get over the fact that, in order to do so, it must act in certain ways on the *occasion* of certain bodily conditions. Earlier in the same Part, Philo has recognised the fact "that matter is, and always has been in continual agitation, as far as human experience

or tradition reaches". But, in going on to consider the hypothesis of a material cosmogony, he considers it possible that matter may exist for a time in a "disorderly" state, after which it may or may not achieve "order"—as if there could be any existence at any time except that of sorts of things, as if "chaos" itself, if it had any meaning, could mean anything but certain things going on in certain ways, i.e., a certain "order". The position is, as indeed we otherwise know, that Hume has not freed himself from dualism, from the doctrine of kinds of existence; yet it is remarkable how little further pressing of the argument of Philo would enable him to do so.

Philo's scepticism, however, his deficiency in logic, comes out most clearly in the well-known passage in Part VII, where he says: "In this little corner of the world alone, there are four principles, *reason, instinct, generation, vegetation*, which are similar to each other, and are the causes of similar effects. What a number of other principles may we naturally suppose in the immense extent and variety of the universe, could we travel from planet to planet and from system to system, in order to examine each part of this mighty fabric? Any one of these four principles above mentioned (and a hundred others which lie open to our conjecture) may afford us a theory, by which to judge of the origin of the world; and it is a palpable and egregious partiality, to confine our view entirely to that principle, by which our own minds operate." The hypothesis of Part VIII, then, is the conjecture of a fifth principle of explanation, that of motion or agitation. But, in the light of the above passage, it is clear why Hume did not see how near he then was to the mark; for, although the question is plainly one of the conditions of existence in general (somewhat confused, no doubt, by the reference to an *origin*), he does not see that these conditions will govern alike the various forms of operation he mentions, that they are not "principles" but particular proceedings of particular things. Philo's question reminds one of the supposition that in certain parts of the "universe" two and two may not be four; indeed, it is even worse, since it is a logical question that is at issue, since we cannot travel away from logic, however distant a system we go to, but the very supposition of such a system is a supposition of complex and interacting things.

Certainly, he quite correctly says (Part VIII) that: "Every event, before experience, is equally difficult and incomprehensible; and every event, after experience, is equally easy and intelligible." Certainly, again, Hume, recognising that

the effect always differs from the cause, could have no logical difficulty in supposing, e.g., that minds arise from the non-mental. We have to remember also that the theory of Cleanthes is the theme of the *Dialogues*, and that Philo is more concerned with weakening the position of Cleanthes than with presenting a position of his own. Nevertheless, design has not been thoroughly dealt with until such a position has been worked out; but that would require the removal of the defects of Hume's theory of causality and, more particularly, the recognition of spatio-temporal continuity and the rejection of the theory of spatial and temporal units.

Once that is done, the removal of the dualism of active and passive presents no difficulty. For although, "before experience", we could suppose these classes to be exclusive or again to intersect, so that, in addition to things which are active and passive, there are active things which are not passive and passive things which are not active, in actual experience we find only interaction, things which act and are acted on. Apart from a rationalistic theory of "natures" (as presented, e.g., by Berkeley) we find no basis for the conception of separate classes of agents and patients, arrangers of phenomena and phenomena to be arranged, designers and the designed. We do not require to go beyond the facts themselves, and of course we cannot logically do so, to obtain an answer to the question how things came to be arranged as they are. The answer to the question how any particular arrangement arose is that it issued from a certain other arrangement, and there is no question of any total arrangement demanding (and being unable to receive) explanation by something more than total, viz., the arrangement of its arranger and itself. Anticipation, as far as it goes, is only one particular relation of the general type in question, and the anticipator is not pure "force" but also "matter", i.e., also acted upon, even in his anticipating, just as the anticipated also acts. This pluralistic conclusion is the upshot of Philo's argument or, at least, it is that alone which would make his position consistent; and it is a positive, not a sceptical conclusion.

THE CATEGORY OF CAUSATION IN PSYCHOLOGY.

By GAIUS F. McINTOSH.

IN an article entitled "What is the Value of Woodworth's Psychology?" in the September issue of the Journal, Dr. H. B. Loughnan discussed various logical and other considerations that are of importance as bearing upon the *stimulus-response* formula, and gave these as reasons for holding that "Woodworth incorrectly describes many facts of mental life when making them fit into his theory". He based his criticism on the first seven editions of Woodworth's well known text-book, "Psychology, A Study of Mental Life", having tried unsuccessfully to obtain a copy of the eighth, ninth or tenth edition. This is of interest, because in the eighth edition (which is the first revised edition, the first seven being similar to one another), Woodworth amends his stimulus-response formula, which instead of reading as formerly,

S — R,
now reads S — O — R.

The new factor O is the organism, and Woodworth now defines psychology as the study of the activities of the *individual* or *organism*, and not of mind. But in his earlier discussion and definition of psychology as the "science of mental life", he said that as "mental" any activity "simply comes from the organism or individual as a whole". So it appears that the change in the definition itself indicates no radical change of doctrine, even though in the new editions the phrase "A Study of Mental Life" is dropped from the title of the book as given on the title pages (although not from the cover). And also the addition of the factor O to the stimulus-response formula is made apparently to prevent misunderstanding of his theory, such as sometimes arose from the simpler formula; and not, as I shall try to point out, to mark a real change of doctrine. There are indeed some signs that Woodworth would be prepared to accept modifications in detail of the physiological scheme laid down earlier; but this in no wise affects the general position—especially what Woodworth takes to be the psychological position, as contrasted with the physiological, which he now believes to be of less importance to the psychologist.

But the treatment of psychology as the science of the organism, I believe, as readers of my article on "The Relation of Psychology to Philosophy" in the June issue of the Journal

will be aware, is both a result of and a cover for many confusions. For, as I tried to show, there is a definite and necessary sphere for study in mind itself; and also mind itself is open to observation, contrary to the traditional view accepted from Hume, which no longer holds once the notion of ideas and of "consciousness" as mental entities or a mental state is given up. And if it be urged in favour of treating psychology as the science of the organism, that mental processes have neural or physical "aspects" or qualities, then the answer is that even so, it would not ordinarily be allowed that the whole or every part of the body or organism is the place of the occurrence of mind—at least the position would have to be argued. And to confuse mind with something which extends beyond it is not the way to bring about clear thinking in a science, or to define its problems sharply. Hence this way of defining psychology leads to the slurring over of specifically psychological or mental questions: in particular, to the neglect of the question of what the mental qualities themselves are. We shall find this is of importance in connection with the operation of the stimulus-response formula.

About the latter, Woodworth says in the eighth edition:

"Sometimes this [S — R] formula is interpreted to mean that if you know the stimulus, you can predict what the response will be; or that if you wish to secure a given response, all you have to do is to apply the proper stimulus.

"But a little reflection shows that the formula, so interpreted, is incomplete; for the stimulus really acts upon the organism, and the organism makes the response. If we use the letter O for the organism or individual, our formula should properly read:

S — O — R, or $S \rightarrow O \rightarrow R$,

and be read that the stimulus, acting upon the organism, elicits a response, or that the organism makes a response when it is aroused by a stimulus. In order to predict the response, we must know not only the stimulus, but also the organism stimulated.

"If you hear some creature stirring in a hedge, and 'coo' to it, it will probably fly away if it is a bird, and rush toward you if it is a dog. If it is a human being, it would be hard to predict the response, without knowing the age, sex, and training of the individual, and the mood he or she happened to be in at the moment. You must know your O.

"You must know the stock of activities which O possesses by virtue of maturation and previous learning. You must know the present state of O, whether awake or asleep, hungry or satisfied, happy or sad, calm or excited. And you must know what O is doing at the moment, what activity your stimulus is

breaking in upon. If he is lost in thought, he will not notice your 'coo' at all, but if he is trying to do something in which he needs help he will respond eagerly . . .

"The stimulus, then, is not the cause of the response—not the full and sufficient cause . . .

"Not only the energy, but the form or character of the response depends on the organism . . . In general, then, the response depends on the structure and condition of the organism. But of course it depends on the stimulus as well" (pp. 226-7).

Woodworth says further :

"Let us apply the stimulus-response formula to the case of the reflex. If anywhere, the simpler form $S - R$, should be sufficient here. In the true reflex, R is strongly and permanently attached to S . Yet it would be misleading to speak of the $S - R$ as an indivisible unit, for the same R may be attached to several stimuli, as we have just seen. When an R becomes conditioned it becomes attached to a new S . Instead of thinking of $S - R$ as a unit, we have to think of the R as the unit, a unit of activity which may be aroused by several different stimuli, in most cases."

(However, the R , even in reflex action, is not a true unit.)

"But the organism, O , has to be considered even here. Though the reflex is a *fairly* dependable response to the stimulus, it fails in certain conditions of the organism. If O is in a condition of terror, with pupils widely dilated, bright light entering the eye fails to give the pupillary reflex. If O is anxious about his knee jerk, the blow on the tendon fails to elicit the reflex. But if O clenches his fist a second before the patellar tendon is struck, the reflex will be unusually strong. If O is carrying a hot dish which he dares not drop, the heat on his hand does not arouse the usual flexion reflex. So we see that, even in the case of reflexes, we cannot predict the response from knowing simply the stimulus, but must know also the condition of the organism" (pp. 231-2).

Now, every one of these considerations was brought forward in the discussion of stimulus-response in the first seven editions ; and for that reason I believe that here there has been no change of doctrine, despite the alteration of emphasis. Woodworth did say formerly (as Loughnan quoted in part in another connection) :

"Stimulus-response psychology is solid, and practical as well ; for if it can establish the laws of reaction, so as to predict what response will be made to a given stimulus, and what stimulus can be depended upon to arouse a desired response, it furnishes the 'knowledge that is power'. Perhaps no more suitable motto could be inscribed over the door of a psychological laboratory than these two words, 'Stimulus-Response'."

But he immediately made it clear, as in the later editions, that there is no question of finding a stimulus-response relation that holds independently of the state of the organism.

Now all this is merely a statement of the general position in regard to the cause-effect relation: the introduction of a factor as a cause will not produce the same result independently of the set of conditions or field into which it is introduced. But once this field is made definite, then the same stimulus or cause will always produce the same result. If in the field F , the introduction of M always results in the appearance of P , then we say that M is a "sufficient cause" of P (within the field F); i.e., All MF are (occasions of) P . And the determinist position is that any occurrence of a change involves a universal proposition of this kind.

DETERMINISM AND SCIENCE.

Since Loughnan would be committed to denying this position we may do well to examine it. In the Socratic method of dealing with a disputed position, or a hypothesis that has been suggested, two steps, one or both of which may be taken in a given situation, are distinguished: (1) the drawing of conclusions from the hypothesis whereby it may be tested in conjunction with other accepted facts, or observation—attempted *verification*; and (2) if the first step results in so far verifying the hypothesis, the attempt to *prove* it by deducing it from a previous position. Now Loughnan has indicated certain psychological occurrences for which he would hold the principle of determinism cannot account; and, hence, in connection with which its attempted verification would fail. And it would be sufficient for the retention of the hypothesis of determinism, if it could be shown that any appearances of non-verification were due to confusions that could be removed. For it is not demanded by the Socratic method that a principle should necessarily be shown to be the consequence of a previous principle in order to be believed even after it has been brought into question, whether by the person himself or by someone else (although this step may sometimes be necessary also in order to produce conviction); but merely that, if the principle continues to be held, deductions made from it should be verified in experience, and that it should not be contradicted by any state of affairs which the person believes. Now, if it were not for the demonstration of the connection between the two, Loughnan might possibly regard it as unjust to identify his attack on the stimulus-response position with an attack on determinism, except in regard to the points raised under the heading of Will, where the question is specially mentioned. But in that case, it would still be a satisfactory defence of the stimulus-response theory

to show that it did give a straightforward account of facts, and that appearances of non-verification could be removed. And it would be quite in order to show, if possible, that certain of these confusions arise from statements by Woodworth or other psychologists, of views which they hold in connection with the stimulus-response position; which explains the objections that McDougall has brought against it—McDougall being one of Woodworth's most vigorous and prominent critics. For it is not necessary in order for a writer to state his central, or any other, position correctly that he should understand all its implications; or that he should not associate false views with it.

But if it is shown that the stimulus-response theory is a consequence of, or can be deduced from, the general theory of determinism, then we have not only verification (allowing for the present that this has been found possible) but proof. And then we have complied with both canons of our method. It is probable that many readers, holding the determinist position, would be satisfied to let the matter stop here. In that case they would regard the demonstration as sufficient. But there are others who would call the determinist position itself into question. And the method of meeting their objection would follow precisely the same steps. There is, again, first the question of verification: and if it is allowed that the principle of determinism correctly describes certain occurrences, then it is a question whether there are any *negative* cases, occurrences which it fails to describe. Now, our examination of this question may be found to admit of simplification at the start. It may be that our objectors only raise the principle of indeterminism in regard to those facts which we have supposed ourselves to have already discussed, *viz.*, in regard to matters where *mind* is concerned. They may hold the principle of determinism in all cases where non-mental occurrences are concerned; and even in some cases where mental occurrences are concerned; but they may except certain mental activities, *e.g.*, what are sometimes called activities of the Will. But if it has been found possible to explain whatever examples of the activities in question, *i.e.*, psychological occurrences, have been brought forward, then if no objection is brought to the principle of determinism in regard to non-psychological events, we may regard the principle as having received *general* verification. That is to say, we may regard it as describing all kinds of happenings.

Here, again, this may be sufficient argument to satisfy the believer in determinism in non-psychological spheres, of the *general* truth of the doctrine. But it is possible that someone may make an objection that there are rival hypotheses which are both capable of explaining events, and which therefore

both receive apparent verification. To take an example from physics, there is the view held by some physicists that natural laws are of the nature of statistical averages of chance operations; and although definitely to accept this view is to deny determinism, still it is possible that an objector might hold that either view gives an equally plausible account of facts. But still without proceeding to the question of finding a proof for one of these rival positions, it may be possible on further examination to show that after all one of the two is not really capable of verification; that we have found a consequence which is inconsistent with our other beliefs, and perhaps with the statement of the theory itself, which is thus found to contain an inherent contradiction. And to restate the theory in such a way as to avoid such contradiction may involve it in consequences which make it lose all plausibility. Or it may be shown that the theory as originally stated has consequences that make it lose plausibility. For example, it may be shown that while certain consequences of each of the two hypotheses can be verified, being the same, the one has additional consequences of a kind that are not found in experience and which from their very nature could not be experienced and could not be verified, while the other hypothesis only demands assumptions of a kind that are found in experience and with which we are acquainted.

Thus the theory that natural laws are of the nature of statistical averages of chance operations, implies that the averages are obtained by these operations behaving in various ways, and whatever ways these may be they will each be of a certain nature and hence definite. That is, given a number of operations along with one another, each acting in a definite way or being of a definite nature (whether the cause of this be taken as chance or not), we get a certain result described as a statistical average or natural law. This might be taken as merely a description of the behaviour of the operations, and not as a *result*; but we can see that while it is a description it is also a result, depending upon a number of occurrences through an extent of time. Hence the "natural law" is not itself a chance operation, but is determined as a definite effect of definite causes. That is, the chance operations are factors in wider situations in which they produce effects according to the ordinary laws of cause and effect. And not only this, but their "effects" are factors or causes in still wider situations in which they produce further effects according to the same laws. And so the process goes on; and it is only of these wider situations that we have any experience. So if the notion of chance is to be retained it can only be in regard to the operations with which the theory starts. But if these are to exist at all they must be definite, for whatever they are we can describe as their nature. We can only take them, then, as being themselves the result of chance—

making the search for the latter into an infinite regress—or say that they are such that they change their nature in a way that is not accountable for in terms of ordinary experience. For in ordinary experience it is a common fact that things (being processes or activities) do change their nature; that is, while they retain certain characters over an extent of time (which enables us to give an activity a definite name, and to recognise it as a definite kind of thing even though it is in process of change) they lose other characters and acquire further ones. But we ordinarily account for this in terms of the factors which are in an activity and those which are introduced into it from the outside. Considering the former as constituting a field *F*, and calling the latter *M* and the result *P*, we are able to say *MF* are (occasions of) *P*, and we believe that what we have observed is a universal proposition. But it is not always necessary to introduce a factor from outside in order that a particular change may occur, as when we apply a lighted match to a candle. The factors already present within the thing may be sufficient to keep it in process and bring about change; for example, the tension of the spring which moves the hands of a clock, the apparatus which works the alarm, or which causes an automatic electric stove to “cut off” with increasing heat. But then we can treat certain characters of the thing as forming a narrower field, and other characters as factors introduced into this field; and so we arrive at a universal proposition just as before.

That we are able to observe universal propositions follows from the fact that characters are never *unit* occurrences. And here it may be remarked that it is the treatment of characters as unit occurrences that gives rise to false psychological theories of the existence of what are called *concepts*, which are supposed to explain awareness of similarities. Knowing that this fire is hot and that fire is hot we are said to form the concept of heat whereby we can compare the two fires and find that they are similar in this respect. Or, knowing that this hot thing is a fire and that hot thing is a fire we are said to form the concept of fire whereby we can compare the two hot things and find that they are alike, that they are both the *same kind* of thing, both fires. But this view overlooks the objection that if it were necessary to form a concept to discover that two things have the same quality, then it would be equally necessary to form a concept to discover that *one* thing had that quality. For the quality occurs not as a unit attached to the thing, but, as it were, spread through it, so that even in speaking of the thing having a quality we have the notion of generality. There is the same problem, admitting of the same solution, in saying that the whole of the extent called the thing has the same quality or, more simply, has the quality, as in saying that a wider extent,

which we may call two things, has the quality. That the parts of this wider extent may not be in juxtaposition with one another does not affect the issue ; for in considering the possession of a quality by one thing we can find parts which are in juxtaposition and parts which are not. The point is that we can treat any two parts, whether adjacent or not, as forming a single extent, by neglecting the intermediate space, and perceive the quality as spread through or flooding this extent. Similarly, what we take to be a thing is not itself a *unit*, but consists of certain qualities occupying an extent and intersecting with one another. Of course, it is not the case when the whole of a thing has a quality that each part of it necessarily has it. There may be a minimum extent required for a quality : for example, a small portion of oxygen forming part of a fire would not be itself a fire. Sometimes the minimum extent may be the maximum extent ; and in that case we cannot, for example, put two squares alongside one another and find that they form a larger square. But the quality square (or squared) while it cannot be divided (or analysed) into, is related to linear and spatial characters which can be treated in this way ; and on this account when we perceive a square—or this is square, we treat and perceive the character as something that could occur elsewhere. There is no question here of having to be presented with a number of examples from which we form a supposed concept, or of forming a concept from one example ; because as all perception involves generality there could be no difference between perception and conception. And so there are no strictly unit characters, that is, characters which do not involve generality. And what we observe in the case of the fire is not this unit of fire and that unit of heat, but an occurrence extended in both space and time of two general characters fire and heat conjoined with one another. Hence it is that on approaching a fire we may not only observe the proposition this fire is hot, but also the general or universal proposition *fire is hot* which may be expressed in orthodox terminology as All fires are hot (things). But although we can observe a general proposition from a single instance of the conjunction of two general terms, it does not follow that we cannot be mistaken in a particular case—just as we can be mistaken in any observation, such as of a particular proposition, *e.g.*, some of these horses are lame, when as a matter of fact none of them are. But if we believe a proposition such as all plants are edible and then find some that are poisonous, we do not rest content with the two particular propositions, some plants are edible and some plants are inedible, but we seek for a factor which will enable us to distinguish between those plants which are edible and those which are not. That is, we seek two universal propositions of the form, all plants which are X (or not-X) are edible, and all plants which are not-X (or X)

are inedible. In such a case we should say not only that X (or not-X) is a "sufficient cause" of edibility, but also that it is a "necessary cause", since plants differing in regard to the quality we have found are inedible.

Now we may consider the case where the field F is a piece of paper into which the factor M is introduced bringing about the result P, the paper being charred. Just as in the case of the fire being hot we were dealing with general characters, so in this case the characters by which we describe the flame and the original state of the paper and its final charred state are general, too; and we observe that the change of state in the paper is due to the operation of certain factors, *viz.*, those comprising its previous state and the flame. And if we found in a particular instance that the paper would not burn, we would seek for an additional factor just as in the case of the inedible plants; this might be found in the fact that the paper was wet. Then we would find that some wet paper burns and some does not, the difference being whether we use benzine or water; also that dry paper in the absence of air, or rather of oxygen, does not burn; and the conclusion we should finally reach would be that all paper burns in the presence of a flame when there is free access of oxygen which water prevents. Each step in the investigation is observed to involve the addition or subtraction of some factor.

This, then, was what we meant in speaking of a thing's change of nature being accountable for in terms of ordinary experience. But the theory of chance operations would imply that given two instances of operations of apparently the same kind we should get different results, and that we should be unable to find any additional factor in the fields which we called the operations that would account for these different results. That is, the theory of chance operations implies that two things of exactly the same nature in exactly the same circumstances may be found to behave differently.

But, as we have said, given their behaviour to be what it is, these chance operations will themselves be factors in wider situations in which they will produce effects according to the ordinary laws of cause and effect, and these effects will produce further effects in this same way, and so on. There is thus a diremption in the theory. Otherwise we should have to assume that at no level could we state universal causal propositions, that we could not, for example, set forth what plants are edible and what are not, or under what conditions fire will burn paper and under what it will not. The theory that "natural laws" are of the nature of statistical averages of chance operations is as a universal statement of laws false. All that it could be taken to assert is that there is a level below which occurrences are indeterminate and above which determinate. But experience

provides us with no example of such a level. All things of which we have experience are complex; and when we observe two things of apparently the same nature producing different results in similar circumstances we assume that there is a differentiating factor involved which we have not observed. And however far we take our analyses we are still dealing with complexes that exhibit a similar kind of logical structure.

In general, we find that the notion of chance operations is confined more or less to alleged *simples*, to unit occurrences in which no variety of factors is postulated to explain their behaviour—or, if the occurrences are conceived at all as complex, their complexity is conceived as insufficient to explain all their behaviour.

Now, it must be said that we can never have a completely simple thing. The arguments which Plato advanced in the second half of the "Parmenides" against the One hold against "absolutes" of all kinds, whether they are thought of as little ones or big ones. For being simples no distinction of this or of any other kind can be made between them. As Plato showed, if we say that a thing is one (or simple) we cannot say anything else about it without contradicting ourselves, and without implying that it is complex. Thus we cannot even say that it differs from anything else; for in order that one simple should differ from another there must be some character above its simplicity or oneness in which it differs. And so the notion of reality as a number of units leads logically to the notion of it as one unit, in which no distinction of any kind can be made; and which, as Plato showed, we cannot even speak of as being one without contradiction. So there must always be some intersection of characters which provide complexity, and we are always dealing with complex things; and the notion that we can reach a stage where the complexity becomes insufficient to explain the nature of a thing's activities is one for which there is no warrant, and is opposed to our whole experience, and opposed to the type of procedure which we see science follows.

The result of our inquiry is that while it may have been objected at the beginning that determinist and indeterminist hypotheses both are *possible* explanations of facts, we have found that the indeterminist position itself implies a limited determinism and a limited indeterminism. But the limited indeterminism is made to occur where it is inaccessible to observation, and, moreover, to be conjoined with facts of a simpler logical nature than any which we can observe. The position is that we can never observe a chance operation, that it is always possible to postulate an explanation for supposed chance operations in terms of ordinary experience; and that when we do observe what are taken to be the *results* of chance operations, these results are treated as following from the chance

operations and from one another in a perfectly determined fashion.

Now the same illegitimate procedure is followed in indeterminist or free-will theories of mind. In so far as the mind itself is not treated as a simple entity in such theories, it is taken as having certain describable features; and then alongside these or behind them there is supposed to be one or more entities the nature of which is treated as simple and not composed of various processes which explain their operation. Further any one of these entities is not described in such theories by any positive features, but merely as that which causes certain effects. That is, in indeterminist theories of mind there are allowed to be certain mental operations which, open to our inspection and having some recognisable and describable quality, tend at least to operate in a perfectly determined fashion; but behind these are other operations which are not so describable, and whose presence can be indicated only in terms of the effects they produce on the former kind. Since such an entity can only be defined in relative terms there can be no possibility of finding any factors present in it to explain its operation.

Of course, in the traditional psychological systems the describable features of mind, too, were supposed to be either simple or analysable into simple natures. But then the difficulty arises of showing how they could operate at all. For example, in associationist or sensationist theories, even though avowedly determinist, it cannot be shown how the sensations or ideas could ever form part of a cause and effect series. The attempt to explain their doing so through "linkages" fails, because being perfectly simple, they have no feature which they could share in common, and their whole nature being summed up in a single quality there is no common ground or medium in which two can partake; the special quality of the one enters into no wider situation which it shares with the other; each is separate and cut off from the other, and there is no possibility of any linkage taking place. And the attempt to explain their causal connection as physiological, fails because even though the physiological occurrence not being simple may permit of connections, the idea being simple cannot share in any way with it. Nor can this difficulty be overcome by regarding the idea and the physiological occurrence as different aspects of a common reality; because this would mean that we should have to regard the idea as having additional qualities, *viz.*, physiological ones, to those we previously recognised, and so as no longer simple. It could be argued that the attempt to treat ideas or other mental entities as simple has never been consistently carried out, that pluralist features have crept into all theories of this kind and given them a certain amount of plausibility. This plausibility it is the work of the psychologist to remove by revealing the contra-

dictions involved in such theories, which can be done by a consideration of the philosophical principles involved. At any rate we can notice that the strict implications of the theory of simple natures in its bearing on causation are the same in psychology as in physics.

But criticism of atomism in psychology has been put forward by Tucker, J. S. Mill, James, Ward, Stout and many other psychologists, especially by members of the Gestalt school. And the arguments which Plato raised in the second part of the "Parmenides" hold as much against mental as against physical units. Thus what we are dealing with in a mental occurrence is something that is complex and involves an intersection of qualities or characters. Now it does not follow that when we know a mental occurrence we know all about it, any more than in the case of the observation of a physical occurrence. But we do observe that mental occurrences enter into relations with one another and with physical occurrences, and know that they must have something in common. That is, we can know that they have common characters and that two mental occurrences and also a mental and a physical occurrence can form part of one situation. This common ground can be provided by taking mental occurrences along with physical as being spatial and temporal, and by saying that mental occurrences can have physical aspects or qualities, or that a physical event can have in addition to other qualities a mental quality. But here we must remember that a minimum extent may be necessary for the presence of a quality, and it does not follow because a complex physiological process is mental that any part of it taken by itself is mental. That is, mental qualities may be taken as having the same logical nature as other qualities. And it does not follow from the view that mental processes are physical that the presence of the mental quality or mental factor in the physical process is without causal efficacy. So we can still speak of minds or of mental processes as having specific causal functions. Indeed, it was because they do have these functions, and interact with physical things which are not minds or mental, that we sought for a common medium for the two, which we found in space-time.

Now, as we have seen, indeterminism invokes special entities standing behind the describable features of mind, and these entities are taken to be indicated by their "effects". But we need not assume any mental operation to be such an "effect" where we can observe it in causal interaction with other mental operations, for example, a conflict between two evenly balanced desires followed by a feeling of apathy. In general, the reason for invoking the entities is to explain what are believed to be departures from a determinist way of behaviour in the positively describable operations of mind. And, of course,

apart from a special faculty of Will, or other special faculties, the whole of mind may be treated as standing behind its operations in this fashion ; and thus we get the treatment of mind, despite the appearances of complexity in mental operations, as itself having a simple, indefinable and unitary nature. Hence we get the view, accepted as dogma in the traditional psychologies, and giving rise to behaviourism, that the mind as such, as distinct from its activities, cannot be observed. Whereas for pluralism with its rejection of simple natures the mind is its activities, and in observing these activities we are observing the mind itself. And by activity, as we pointed out earlier, the pluralist means a field or complex in which certain characters are constant, whereby we can give the complex a name and regard it as *one* thing, but which is undergoing change in other characters which it loses or gains, or which are being distributed differently through the field. But in addition to the rejection of simple natures there is the further point that theories of higher natures or different levels of reality can be rejected—such as are involved in the notion of mind or any of its faculties or parts, or special entities, such as the Will, standing alongside or behind its activities. Because as Plato showed, this time in the first part of the “Parmenides”, in order that any connection should occur between two levels of reality the latter must both be taken not as two levels but as being on a common single level of reality, and thus as sharing in common features. Otherwise there could be no reality which the connection between them could be said to have, it would simply not exist.

Hence if the theory of indeterminism is to retain any possibility of being maintained it must give up the notion of higher powers, that is, give up the notion of the mind or of a faculty standing behind its activities, and hold that whatever entities are invoked to explain or allow for indeterminism instead of acting above what are otherwise explainable as determinate activities, act on the same level with them. And then it will be seen what the theory really implies, *viz.*, that in psychology as in physics those activities which we are able to recognise and describe, that is, those activities which we can observe, themselves conform to or are describable by the ordinary laws of cause and effect ; but that there are other activities inaccessible to observation, treated as simples, that is, as not containing sufficient factors to explain their activities in a given situation. Indeterminism is thus seen to depend upon the postulate that the conditions of the kind we have described as obtaining in the sphere that is open to observation, do not obtain in a sphere in which observation is impossible. It holds that there, when two things of apparently the same nature behave differently in the same circumstances, there is no

differentiating factor for us to find, even if we could observe it ; such as we said we should look for in plants if after forming the hypothesis all plants are edible, we found some that were inedible. To sum up, indeterminism depends for its plausibility on the notion of things of a simple or relatively simple logical structure, of which we have absolutely no experience in observation ; and so, to let the theory keep any plausibility at all, these simples are always supposed to exist under conditions in which they are not open to observation, for example, as prior to observation or in a realm above it.

We have seen that determinism is bound up with the possibility of universal propositions of the type that within a certain field *F* the introduction of a factor *M* results in the appearance of *P*, that is, All *MF* are *P*. Consequently, the discovery of such universal propositions was seen to constitute a verification of determinism. And it may be further stated that even universal propositions that do not directly involve causal relations, but the universal occurrence of one property in conjunction with another (as in the examples, all spatial occurrences are temporal, all men have minds, all neuroses are accompanied by repression), show that change or process is not merely haphazard. For if it were merely haphazard, propositions such as these would be false. There would be the possibility at least of neuroses which did not involve repression, beings resembling men in all respects except mentality, and so on, including all the wonders of Mandeville's " Voyages and Travels " with diamonds that do " grow together, male and female, and are nourished by the dew of heaven ". And without foolishness Desdemona might seriously incline to hear of

" The Anthropophagi, and men whose heads
Do grow beneath their shoulders ".

On the other hand, the truth of universal propositions shows that there is a limitation to the varieties of things that can be brought about, and so of the types of changes that are possible ; and this implies further that changes occur under definite conditions. There is thus no possibility of a universal indeterminism ; and we have said sufficient both to show the lack of plausibility of a limited indeterminism, and that it is opposed to the development of the postulates under which science proceeds. Thus passing from the question of verification, or the examination of consequences, to the question of proof, we can exhibit determinism and the truth of universal propositions as a consequence of the postulate that we can make connected statements about things, and hence of the very possibility of science. And it may be said at once that even an anti-intellectual doctrine, for example, the view that all true propositions are particulars, could only be stated and argued by making use of the universal form that it denied, that is, could

not be stated without contradiction. This was what Plato pointed out in regard to all forms of Monism, *viz.*, that the theories could never even be stated, and if they might, even so could never be supported.

Hence determinism is bound up with the way we speak about things, and with the very possibility of the making of connected assertions. And science itself consists in the search for universal propositions. An hypothesis is formulated of the form All X are Y, and is tested by comparison with individual cases of X that we can observe; if these are all found to be Y, then we say that so far the hypothesis is verified. (And as we said, if it seems possible we may also attempt to prove it by deducing it from another universal proposition that we hold.) But if at any time we find not only X which are Y, but some X which are not Y, then we seek for a further term Q such that we can say All QX are Y. If after apparent verification it should turn out again that this hypothesis is falsified, we seek another term R such that All RX are Y. But we also believe, and it follows from this procedure since we can treat QX as a single term, that there is another term S, for which we can seek, such that All S(QX) are Y. In terms of the theory of classification QX is a genus and S is a difference marking off those QX that are Y from those that are not Y. In turn Q is a difference marking off the species QX of the genus X. So we can take the two differences together and say All (SQ)X are Y. SQ is now the difference marking off the species of X that are Y, and may be written more simply as M, SQ or M being the term for which we originally sought; and All MX are Y is the hypothesis for which we sought, and now believe we have verified. This means that whenever an hypothesis of the type All X are Y is falsified in certain cases but verified in others so that Some X are Y, there is a proposition of the type All MX are Y which is true. But MX is a species of the genus X, and we may call it N. Hence we see what is at the root of all scientific investigation, and what the postulate of the very possibility of science implies, *viz.*, that the truth of a particular proposition, Some X are Y, implies the truth of a further proposition, All N are Y.

VERIFICATION.

We must now examine in more detail the assumption we made at the beginning of the argument that verification of the stimulus-response position is possible. It is assumed further that the existence of positive cases is not in question, that we do find instances where given a definite state of mind (Woodworth would say organism) or a definite mental process as field, the introduction of another factor, whether this be another mental process or a non-mental physiological and physical

process relating the mental process to a state of the interior of the body or to something beyond it, results in a changed state of the mental process in question. If not exactly stated, this is at least implied by the instances that Woodworth gives in the passages quoted earlier. And we must remember that Woodworth says that there is no question of taking the stimulus as producing the response independently of the state of mind (or, as he puts it, organism). "The stimulus, then", he says, "is not the cause of the response—not the full and sufficient cause." Although, as we pointed out before, we can understand the logical nature of the operation by taking a particular mental state as a constant or field in relation to which the introduction may be taken as the "sufficient cause" of a change. But, even if the verbal expression of the operation may be cumbrous in some cases, the logical principles involved may be equally illustrated by taking the process called the stimulus as the constant or field, and the mental state formerly called the field as the additional factor. And this factor in the field constituted by the stimulus may now be taken as the "sufficient cause" of the change. We may see this more readily, perhaps, in the case of the interaction of two mental operations, for example, a struggle between fear and anger. In the terminology we have used, either may be described as a "sufficient cause" of a change in the other. The fear grows less on account of the anger, and the anger becomes intensified on account of the opposition of the fear. Or the fear is stronger than the anger and the two combine into hate. The fear may be taken as the constant in which anger produces modifications or complications, or *vice versa*.

Loughnan might object that in the quotations he gave from Woodworth stimuli were defined physiologically and not psychologically. That is so. But allowing that Woodworth's notion of what constitutes psychology is somewhat confused by the notion of such action issuing from the whole of the organism and not merely from the mental parts of it, that is, those physiological processes which have the additional character of being mental—Woodworth does give a psychological treatment as well. For example, in the passage from which Loughnan quoted on "central stimuli" (p. 48) there are two paragraphs dealing with the psychological aspect; and the second part of the quotation is preceded in the book by the words "There is no special difficulty with the notion of 'central stimuli' from the physiological side", with which Woodworth then goes on to deal. I do not mean to suggest at all that Loughnan's form of quotation is unjustified—but merely that Woodworth also gave what he took to be a psychological treatment of the formula, as opposed to a physiological one. And in the new editions he now considers that physiology is of less (but by no

means no) importance to the psychologist; though his physiological explanation would be much the same, except that he is prepared to accept alternative explanations of motivation to that of dammed up energy in a nerve centre in the brain, to which, as it happens, Loughnan objected.

However, the physiological question is one to be faced; and here it may be suggested that there are logical considerations that would lead us to regard Woodworth's account as possibly over simple. Loughnan has pointed out a similar over-simplicity in Woodworth's discussion of the laws of Association. Woodworth says: "In saying that two or more stimuli arouse a single response, we imply that *there is already some rudimentary linkage between each stimulus and their common response.*" And Loughnan replies:

"I suggest the comment that two causes may in combination produce a result with which neither of the causes when taken singly is linked even in a rudimentary way. For example, a draught of water saves my life when I am dying of thirst; but separate draughts of oxygen and hydrogen are utterly useless—neither the oxygen by itself nor the hydrogen is 'linked in a rudimentary way' with the quenching of my thirst."

The point is that we cannot take psychological or physiological occurrences as mere collections of units (even if the latter are not taken as absolutely simple units like some of the units of traditional psychology). The reason is that a minimum extent may be required for the presence of certain characters, that are greater than any of the units; and so if we take psychological or physiological operations as entirely a matter of units we must regard these characters as causally ineffective. But either we should have to say then that the characters extending beyond the units have a different logical nature from those found within them, or that the more extensive of the latter are also causally ineffective, and so on, until we should have no alternative but to come to simple natures (or true units) the notion of which we criticised, or else we should simply have to say that causation does not exist. And so, as Gestalt theorists have also argued, the attempt to give a completely atomic treatment of facts leads to absurdity. Instead, we have to regard causality as an interplay of factors, some of which require for their introduction or existence a greater extent than others. Or instead of treating the question in terms of extent, it may be treated in terms of complexity; although care is needed in doing so, since we have regarded all things as infinitely complex. But the question is not merely quantitative, it is also qualitative; and the point is that given a certain conjunction and order of characters over a certain minimum extent we get the appearance of a new character which acts as a new factor in the causal situation. Hence we can understand the logical position that is involved

when given a certain complexity of physiological processes there is the appearance of a mental character. The new character, although a quality of a certain physical situation, is not necessarily a quality of any part of that situation taken by itself; and also it is not a mere addition to it, but rather a further complication of it. Nor, although some non-mental qualities that are always conjoined with the appearance of mind can be found in the absence of mental qualities, can the one be considered in any other way as logically prior to the other. Thus we can equally speak of a mental operation having physiological and other non-mental characters, and of a certain complexity of physiological processes as having mental characters. Hence, even when we are speaking of the same *thing*, we can differentiate between psychological characters and physiological and other non-mental characters. This is only what is true of any complex involving a variety of characters. And so we can still discuss psychological questions without explicitly introducing physiological questions, even though our statements will have physiological implications. Similarly, some physiological processes can be discussed without introducing mental questions; but as mental characters enter as complicating factors into wider physiological situations, in these cases no satisfactory explanation can be given of what happens that does not take into account these complicating factors.

While, then, it might be found impossible to verify the stimulus-response formula in connection with the atomistic physiology with which it has usually been associated, still we have indicated the type of physiological verification that would obtain if we can give it psychological verification.

We have seen under what conditions verification would hold; and the question is now one as to whether negative instances can be found. Now Loughnan has advanced four types of reasons why such cases can be found: (1) The first is that some mental activity involves thought of what cannot be stimuli: relations, implications, possible occurrences, obligations, meanings. But supposing Loughnan is correct in that such are not known to us through peripheral processes, what happens? They must be known by some mental act. And the answer is simply that the mental process which knows them is aroused by other mental processes. Or, alternatively, that the mental process which knows them is aroused by processes from the periphery whereby it is related to various objects, not relations, and undergoes various modifications and that it is *these* that occasion it to know the relations. Thus Loughnan says he can know the difference in size between two pipes by an act of comparison, but that this act is not a response to a stimulus for *difference* cannot be a stimulus. Very well; each pipe provides a stimulus, and the mental acts which these occasion

act as central stimuli to a third act of comparison. What could be simpler? But Loughnan will not have it. Why? Because there are no stimuli corresponding to, in the sense of resembling, the relation. But the stimulus-response position does not stand or fall by the notion that every mental act must be like its cause, but that every mental act must have a cause. Love, for example, love of a pet lamb is not like a pet lamb. (And the act which knows the lamb may not be like a pet lamb, either. But even if it is held that mental acts are somehow like their objects, it does not follow that the thought of a lamb may not be aroused by that very different thing, the thought of a lion! The difference here is that according to this type of theory the first time of thinking of the lamb was due to a peripheral stimulus, but not the first time of thinking of the relation; but this cannot be made an objection if, *ex hypothesi*, the knowledge of the relation cannot be directly due to a peripheral stimulus.) Loughnan says further: "No catenation of nerve modifications, corresponding to various thoughts, can produce an entirely new thought. The most it can do is to bring into consciousness an old one, or a combination of old ones." But is this criticism of Woodworth or of the theory? Sometimes it is kindness to state an opponent's position for him. And we have quoted from Loughnan to show that a combination of stimuli can produce a result that is not connected with either of them separately.

But since the publication of James's three famous essays in 1904-5, reprinted as I, II, and III in "Essays in Radical Empiricism" (and the third also as Appendix A in "A Pluralistic Universe"), can we treat lightly the view that relations are as much existences in the everyday world as the things they are taken to relate? And is not *implication* a fact in this world? Is it not actually the case that when $A > B > C$, this *implies* $A > C$ even if no one knows it? And if one pipe is larger than another, is not this a fact, too? Are not the relations, difference, greater than, implication, found here? And cannot they be stimuli? Loughnan says: "The difference in size between the two pipes is not itself a *third* object, providing a third stimulus." But is it not a third object, and does it not provide a third stimulus? The position is that we cannot merely count the pipes "one, two", and then have done with it. What we have before us is not two units taken separately, but a complex situation, and there are many factors in this situation that it would seem we may count as objects and as stimuli. The distance between the pipes, their position, and their other spatial relations, including their difference in size, are not these factors in the situation, and to be counted as much as the colour of the pipes and their other qualities? And cannot they be stimuli? There are not merely two processes going on from the pipes to the brain, one from each pipe, for do not these processes

bear a certain relation to one another and form part of a wider process? And if there were merely two processes unrelated except that they enter the brain together, could not the two taken together there produce a result that neither could produce by itself, and together form the stimulus for the perception of *difference*?

We have already discussed the position in regard to concepts and knowledge of principles. But what of possibilities? Does not knowledge of them mean at least the relation of a mental act to actually existing things, qualities and relations, even if the qualities are supposed to be in relations in which they are not? That is, is not the most we can have in imagination not "entirely new thought", but "a combination of old ones"? So that, after all, we can give an explanation of knowledge without the hypothesis that some knowledge (other, of course, than knowledge of mind) is of a specifically mental origin, that is, is not related to its "object" by physiological and physical process, as other knowledge is.

(2) The second objection is that mental activity involves effort or striving on our part, for example in trying to find relations. I cannot see that Woodworth ever denied this. (Nor, by the way, does it show that relations are specially mental, for effort may be required to discover any given character in a situation.) However, as McDougall has pointed out, Woodworth finds much difficulty with the notion of striving because he hesitates to admit the causal efficacy of psychical activity. We have already had reason to see that this follows from his atomistic physiology, and have discussed the conditions that make striving possible in so far as it means the causal efficacy of psychological factors. And further explanation is connected with (3) the third objection, that mental activities involve guidance and selection. But Woodworth did not deny this either. In fact he discussed this very question at some length in one of the sections on the stimulus-response formula (p. 68f). Our solution is that guidance is not conducted by a unit entity, a mind or faculty conceived as standing behind mental operations; but, as any pluralist theory must maintain, by the operations themselves. That is, a certain mental operation will, according to its state at the time, tend towards certain types of action on other operations, mental and non-mental, and to be responsive towards certain types of stimuli and not to others. For example, if we are trying to solve a problem, we may at one time believe that a certain kind of fact will help us, and seek for and be responsive to stimuli of this particular kind; whereas later we may seek for another kind of fact and be responsive to that. But the mental operations themselves are not to be taken as so many units; and so we can have mental operations combined into various more complex

systems, which we may describe loosely as dominated by various "desires", although these desires are combined with others less strong, but which are satisfied by the same "ends". Of course, "desire" is as much a relative term as "end", and in any particular exact description it would need to be replaced by a positive account of the qualities of the processes; and whether the reader holds an intellectualist view of mind, or like the writer an affectional, it might possibly be agreed that the positive qualities of such wider situations would be feelings. However, the answer to this more special question is of less importance here, as it is rather the logical or formal characters of mental activities that are being discussed. Our treatment goes beyond Woodworth's, who, as McDougall again points out, fights shy of fundamental analyses; but he fully realises that mental activity is not a mere random stimulation of nerve centres in a chain just as they happen to occur, and he employs the notion of "mental set" to illustrate the facilitation of some processes and inhibition of others that occurs when a mental activity is unsatisfied.

(4) The fourth reason that Loughnan gives is that determinism involves rejection of individual responsibility and moral obligation. This argument would only provide us with a *negative instance* if we can show both that this rejection is implied and that the factor which is rejected by the theory exists. Now with regard to responsibility, the first condition fails. As Woodworth says:

"If free will means that the individual's action at any time depends neither on the environment confronting him, nor on his own constitution as developed out of heredity by his past activity—then free will means the action of nothing in a vacuum. On the other hand, if determinism means that the environment wholly determines the individual's response, as if the individual himself counted for nothing, then determinism is only one degree less absurd than free will, and is talking of the action of nothing though not in a vacuum" (p. 182, 8th ed.).

But determinism means nothing of the sort. There is no question of taking one factor as logically superior or inferior to the other. Determinism does not mean that the environment entirely determines the mental response, any more than it means that mind entirely determines the environment. Each has its own nature and acts out of that nature. Each acts on the other and is acted on by the other. But however a mind may be acted on, the resulting state is *its own state*, and any act that it performs is *its own act*. And it is essentially because we are a complex of interacting processes that we *are* responsible for our acts. But with regard to obligation the second condition fails—unless we take obligation to mean that we can follow only a certain line of action if we are to arrive at a certain result, as

when we say we are obliged to get up in order to switch on the light, or that we are obliged to lead a certain kind of life in order to produce a certain good activity of soul ; but there can be no " absolute " obligation that is neither an actual relation between things, nor a positive character of things.

The conclusion is, then, that Loughnan's criticism does not provide a negative instance of the stimulus-response formula ; although we have seen reason to hold that its operation requires a rather more complex treatment than Woodworth has given it. In particular we have seen that his physiological treatment suffers greatly from over-simplification, and that his psychological treatment suffers because he has not taken philosophical principles sufficiently into account and made an examination of the logical conditions in which minds are found, and formed a definite theory as to the nature of mind. And however much I may disagree with many of Loughnan's criticisms, I cannot but regard his article with a great deal of sympathy, because he does realise how important it is for psychology to face up to philosophy.

THE NATURE OF INTELLIGENCE.

By J. A. PASSMORE.

IN the history of thought, retrogression succeeds development; and there is no more potent factor in effecting this reaction than a failure to appreciate the real reasons for the rejection of a theory, the basic logical issues. Too often, only the word is defeated, while the theory lives on in another guise. The doctrine of faculties is an outstanding example of a theory which changes in externals but, in essence, continues to occupy its position of dominance, however many assaults are made upon it. Thus, today, the faculty theory is generally described as "discredited"; and yet everywhere we hear of intelligence, character and temperament, of abilities to do this and capacities for the other. Capacities and abilities are easily recognisable as faculties; but the position in regard to character, temperament and intelligence is not quite so obvious. Yet seek for details and you will learn that intelligence is the capacity of the organism to adjust itself, character its capacity to will, temperament its capacity to feel.

Faculties are commonly assailed on two grounds, it being said first that to assert their existence is to deny the unity of the mind, and secondly that the faculty theory is bound up with structuralism, the two lines of attack being closely connected. The first objection only has force if it can be shown independently, and this is not done, that the mind *is* a unity. In fact, those who accept the unitary theory of mind are faced with special difficulties, since they are unable to explain how mental conflict and introspection are possible. Mental activities enter into various relations of opposition and support, inquiry and repression, with one another; and the psychologist has either to explain away these facts by the adoption of a thorough-going Idealism, abandoning the possibility of presenting psychological theory in so doing, or else must deny the theory of a unitary self. His boasted independence of philosophy does not carry with it an independence of logic. The truth of a doctrine carries with it the truth of its logical implications; and to leave the implications to scientists in another field simply amounts to shirking the difficulties of the situation, in fact to obscurantism.

In the same way, the psychologist has to face up to the implications of his functionalism. Functionalism is bound up with teleology, with the identification of quality and aim, so that it is regarded as an adequate description of a thing to say

what it strives after or, as it is more commonly put, what it is for. Thus the psychologist comes up against the problems which have confronted any teleology from that of Socrates onwards. We still have to determine what it is that strives, or is for, and, again, unless what it is for is itself a structure, we have not determined the activity even in terms of its object. Thus either we fall into the morass of dualism, with mind as function (activity) and things as structure (passivity) and with organism occupying an anomalous position between the two, or else we are left with a world of functions, in which nothing has characters.

The rejection of functionalism does not imply the acceptance of structuralism, in the special sense in which that term is employed, *viz.*, that mind is a collection of passive "ideas". The pluralistic view of mind is perfectly compatible with its activity; indeed it *involves* the treatment of mind as a society of active forces. The assertion that mind has a structure means only that it is a definite sort of thing, that it has characters of its own which can be examined quite apart from their effects on other things. The question is still left open what sort of thing it is, what activities go on in mind; and this is precisely the question which psychology has to solve.

The fundamental objection to the theory of faculties is that it pretends to give information about the mind, to determine what has relations and not merely what relations it has, without really advancing matters in the least. To discover that what knows is "consciousness", or that what reasons is "reason" or what judges is "conscience", is of no avail while consciousness can only be defined as what knows, reason as what reasons and conscience as what judges. In all these cases our relation has simply been turned into a quality, so that instead of determining what has the relation, we have merely come to the conclusion that there is something which has the relation.

It is further assumed that if a thing has a relation, there must be in it some special force to have the relation, so that if a thing judges it must have a "capacity to judge", if it knows it must have a "capacity to know" and so forth. This theory, especially as it relates to causality, was thoroughly demolished by Hume, but, of course, psychologists have the queer notion that no philosopher after Locke is worth reading. Hume argued, in effect, that if to have a relation implies the presence in the thing related of a capacity to have the relation, then it is this capacity which really enters into the relation, and consequently it must, by hypothesis, require a further capacity and so on in an infinite regress. If this argument sounds too much like that quibbling for which philosophers are noted, then psychologists might take account of the further argument that, unless we have had prior experience of the operations of a

thing, we find it quite impossible to say what it can be used for, so that it does not appear that we can distinguish any capacities or powers in the thing itself. The doctrine of "capacities" and "abilities" would make of mind a collection of "powers to do things" without its being shown that these powers do exist in their alleged subject, and without their being differentiated in character. It is fashionable to attribute these powers to the "organism", so that when we inquire what carries on a certain activity we are informed that it is the organism, while on the other hand, no characters of the organism are indicated except that it carries on these various activities: a specious appearance of unity is provided by reference to the organism, it being understood that the capacities are capacities *of* the organism and not powers *in* the organism. The psychologist generally wavers from one to the other of these two conceptions, having to adopt the first to give any positive notion of what has the capacity, and the second to show how there can be relations between mental processes. Neither can be adopted consistently, but the first at least has the advantage of making it clear that when we say "X is capable of Y", we mean only that "X can do Y", that Y is one way in which X can behave, and that the question what X is has not been solved. Thus it leaves the way open for psychology, even though the solution in terms of "organism" cannot consistently be maintained.

The doctrine of "intelligence" provides one of the most prominent examples of "capacity" or "functional" psychology and makes it clear how theories of this sort develop, namely by a confusion between a phase of a process and a cause of a process. We do commonly refer to "intelligent behaviour", but this does not imply that there is a special faculty which carries on this behaviour. It is even doubtful whether we can properly characterise any special type of behaviour as intelligent, the tendency being to describe in that way any behaviour of which we approve. This is marked in Thorndike's definition of intellect as "the power of good responses from the point of view of truth and fact" (*Journal of Educational Psychology*, March, 1921, p. 124). Good is certainly not being used in an ethical sense here; and if we attempt to define it more closely we discover that it simply means "appropriate", or "approved". A common definition of intelligence is in terms of "problem-solving", but if this view is accepted then all behaviour is intelligent, since any form of behaviour is the solution of a problem, the object-finding of a motive. If we inquire what $2+2$ equals, and the answer is given as "Sydney", then indeed *our* problem has not been solved, but the utterance of the word Sydney must have solved some problem in the mind of the speaker. It is a response to some situation, even though it is not the response we require.

To take a less disputable example, if we substituted human beings for rats in the maze experiment, we should become rather indignant if the prisoners proceeded to work out algebraic problems on the walls, or to examine the effectiveness of our carpentry. The thing to do, we should feel, is to get out of the maze. Yet the other activities would equally be the solution of problems, even though not of the problem we desired to be solved. This raises two points : first, that those who claim that "intelligence" simply means ability to answer tests are on the right track in seeing that we commonly mean by intelligent conduct behaviour which solves our problems in our way, and secondly, that we never come across simple stimuli which have only one character and present only one problem. The first of these points has as a consequence that we consider behaviour intelligent in relation to our demands (a view which is given support by the extreme difference of opinion on the matter of intelligent behaviour) and not in terms of its intrinsic character, while the second point casts doubt on the "precision" of intelligence tests, a doubt which, as we shall see, is deepened by a further consideration of these tests.

In an endeavour to ensure that *their* problem is solved, psychologists commonly place the rats in the maze under special circumstances. The rat is starved and food is placed outside the maze, or he is excited sexually and hindrances are placed in his path to satisfaction. Now, if there were anything describable as pure intelligence, which did all our problem solving, our procedure would be a curious one, for we should not expect to meet under these circumstances that calm deliberation and survey of various lines of activity which are commonly associated with this faculty. We are, in fact, simply seeing whether the rat can behave intelligently, and we recognise that drives of the kind we are arousing can carry on inquiry. We do not appeal to a special "capacity to inquire"; we see whether the rat can inquire; and we recognise that hunger is one of the forces that can inquire in this way. The behaviour remains much the same whether hunger or lust is operating: so that it appears that inquiry can be carried on by various motives, even if not by all motives.

When these facts have to some degree been recognised, the tendency has been to treat intelligence as a faculty of a special sort, so that we hear of it as "the servant of instinct". It can only be evoked into activity at the behest of other forces, so that it cannot *initiate*, but must remain in inactivity until expressly called upon. Now, apart from the fact that we have absolutely no evidence for the existence of such a faculty, so that the assertion of its existence is at best a mere assumption, it is quite impossible for anything of this purely potential sort to exist. The mere presence of intelligence in mind would

imply that it has various connections with other mental activities, spatial proximity implying interaction. If intelligence exists at all, it must have characters of its own, and consequently cannot be a pure servant. The real situation is that intelligent behaviour is simply the way, or at least one way, in which instincts operate; and that there is no special faculty which carries out this intelligent behaviour.

The opposed view, in which Intelligence is the direct descendant of Reason, treats it as something strictly impartial, which regulates instincts in a special way; or points out to them the consequences of their action. There is an immediate contradiction here, the recognition that reason has a definite way of behaviour conflicting with the treatment of it as possessing a peculiar impartiality. In other words, an impartial judge could never advise one line of action rather than another; it is only if it is a thing of a definite sort, with definite characters (not merely "that which judges") and definite interests, *i.e.*, seeking definite objects, that it could possibly judge. In any case, we find that no mental activity has a special monopoly over regulation and admonition. We may speak of a life dominated by prudence, or by the love of truth, or by fear where one force tends to assume a special position of control in the mind, but we always have alternations, various forces obtaining dominance at different times and checking other forces.

There is one force, the existence of which explains some of the plausibility of theories of reason, and that is the love of truth, or spirit of inquiry. How far this can be equated with curiosity is a matter for further investigation, but it seems at least that some of the odium attaching to curiosity which seems to differentiate it from the generally approved love of truth may be accounted for by reference to the old confusion between mental activity and overt behaviour. We tend to talk about "prurient curiosity" and "the curiosity of scandalmongers"; but here we really refer to the inquiring behaviour of the prurient, and of the scandalmonger. The forces operating in the mind which carry on this behaviour are quite different *qualitatively* from those which actuate the scientific inquirer. Even the behaviour, the completed work, differs in character—a scientific work on sexual relations obviously differs from pornography—but the difference between the forces carrying on the inquiry is even more marked. In any case, there does seem to be a special type of mental activity, an intense desire to discover truth, a desire as emotional and as particular as any other desire, but distinguished qualitatively from other desires which can inquire and which can even discover truth.

If this theory of intelligence is accepted, in which the faculty disappears, and we speak only of intelligent behaviour, certain current opinions regarding intelligence tests will require

to be considerably revised. The extreme concern with these tests, and with experimental work generally, has been markedly detrimental to psychological science. Behaviourists may well jeer at their "spiritualistic" opponents; academic psychologists, for the most part, place a trusting faith in the existence of "something more" than is admitted by behaviourists, but then go happily ahead to examine the conditions and results of mental activity in exactly the behaviourist manner. With their unitary theories of mind, and their identification of science with laboratory work, they are quite unable to answer the behaviourists' criticism of introspection. Consequently, they are compelled to bifurcate their theories, setting on one side a dubiously optimistic theory of free will and the "something more", and on the other side their "safe and certain" laboratory work. Thus psychologists who are opposed in every psychological issue can yet behave in exactly the same way. Psychology and physiology are no longer differentiated. Certainly, we sometimes hear of the "psychological interpretation", but such interpretation is quite impossible unless we have direct knowledge of mental activities, on the one hand, and overt behaviour on the other. Thus, it is a condition of the fruitfulness of psycho-galvanic experiments that we should have the prior information that certain emotions manifest themselves in definite forms of behaviour, and, especially, that certain situations, evoked by words, are commonly connected with definite emotions.

Intelligence tests manifest with special clarity this combination of practical unanimity with a theoretical divergence amounting to anarchy. Spearman's doctrine of "g" and "s" has many supporters; but his own precautions in establishing his theory have not always been so much in evidence in his followers. If we take up the attitude that "g" is purely a quantitative factor, then although we have to make it clear that this really amounts to denying that it is a factor at all, we at least will not make the mistake of considering that it is possible to found psychology on the statistics of behaviour. The special development which equates "g" with cortical energy and "s" with the efficiency of neurons does not really advance matters in the least. Psychologists who complain that they cannot find this "general energy" are in the right; for indeed, we never come across pure energy, but only things which exert a certain force. The reference to "energy" is simply an attempt to make it appear that what we can do is in some way in us.

The relation of theories of this kind to psychology can be made clear by a parallel with bodily performance. If we were to set up various tests of muscular strength, then we might discover first, that different muscles vary in strength, and

secondly, that there was a high positive correlation between different muscular sets. We could say, then, that the subject had a certain strength, and secondly, that various muscles were of different degrees of strength. This would not imply, however, that there were two factors, physical strength and muscular strength; the correlation occurs because the muscles have similar conditions, merely as occurring in the one human being. We might also discover that the correlation between the strength of one muscular set and the "general ability" was particularly high, so that a test of this set would provide a fair guidance for the determination of physical strength. This information, in terms of force exerted, would be of no use to the anatomist. What he requires to determine is what muscles there are, and how they are related. In exactly the same way, the psychologist has to discover the character of the forces the strength of which is being tested. Knowing what muscles there are, we can arrange to test them; but the procedure of the psychologist, who uses the tests to determine the kind of forces operating, appears on the face of it curious. His claim to be making a random selection is open to serious objection, for it is impossible to determine whether a selection is random unless we are acquainted with the whole field of mental activities. Even then definite considerations dictate the choice, so that the claim to randomness rests on a shaky basis. A genuine selection could only be based on a knowledge of mental activities and their way of behaviour (even assuming, what I would deny, the specificity of a motive's behaviour), so that mental testing requires for a scientific basis a thorough knowledge of minds and their ways of operating.

There are several reasons for regarding the tests as at present constituted with the gravest suspicion. For one thing, psychologists display a pathetic faith in the coherence theory of truth, which they employ with the utmost ingenuousness. New correlation is but old coherence writ large. Psychologists adopt a curious dialectic in which the truth of certain judgments is used to prove the truth of other judgments, which are then turned around to deny the truth of the first type of judgment. If we wish to decide whether a certain test shall be included in a set of questions, then two processes are employed, the test being correlated with other tests or else with outside estimates. Yet these outside estimates are condemned for their unreliability! Nevertheless, the sole criterion of an intelligence test is afforded by "success" in other ways, so that it is assumed that this success is itself a sign of the presence of intelligence. When a test has been worked out in this way, by this sort of correlation, we are then given to understand that the tests reveal the successful to be more intelligent than any other class!

A close examination of the nature of the tests reveals, further, that their claim to generality cannot be maintained. First of all, it is impossible to test ability except by testing knowledge. Admittedly a person can have potentialities which have not yet been fully developed; but we can only determine whether he is "a mute, inglorious Milton", if he behaves in certain special ways. A child who has been inefficiently taught may lack certain scholastic knowledge; and a test of knowledge of a relatively non-scholastic type may reveal him to be well equipped in this respect. If, then, opportunities for acquiring non-scholastic knowledge are assumed to be equal, we can say that he reveals himself as equal to his fellows where the conditions are the same. The catch lies in the condition; and intelligence testers seem to make the bold assumption that this condition always holds. We hear much about intelligence tests as discounting inequality of scholastic opportunity, but it is not realised (in spite of Burt's canal-children) that the opportunity of acquiring the sort of knowledge tested by intelligence tests may be unequal; that in fact it will vary with the social environment of the child.

Considering a test in detail, we may take as one extreme Dr. Phillips' three year old test. The child is asked to point to his nose, eye and mouth, to repeat a certain phrase, to repeat two digits, to enumerate objects in pictures, to give Christian name and surname and to name his or her sex. Now, all this test can determine is whether the child has picked up the sort of knowledge that a child does normally pick up at that age in a certain environment. These things are learnt, and this learning may occur in two ways—by imitation or by direct inculcation. The test cannot, then, determine whether the knowledge has been acquired by the questing spirit of the child, or as a result of the special attention which has been paid to him. A child who has had a lot of attention paid to him, who is used to hearing people talk about him, and who has some acquaintance with picture-books, will find these tests easy. The most we could say, then, is that any child in that environment (the further assumption being made that he is interested in the test) who fails to pass is probably dull; but we certainly could not say generally that the unsuccessful child must be dull.

The later tests are fundamentally of the same character. Questions which demand the interpretation of illustrations, the repetition of digits, the rearrangement of sentences, all depend upon the prior acquaintance of the subject with pictorial, numerical or verbal materials. Vocabulary tests quite evidently depend upon environmental factors—the child of the professional man, unless he is mentally deficient, is bound to have a wider vocabulary, at least in the field of vocabulary employed by the tester, than the children of the ordinary worker. And synonym

and opposite tests evidently imply familiarity with words, just as analogy and completion tests are of a literary character.

Intelligence tests, then, are simply a special sort of new type test in which the material employed is of a non-scholastic, but not unlearned character. As such, they have all the advantages and equally the disadvantages of those tests. The main advantage seems to be that they are easier to mark with accuracy; the disadvantage that the essential character of all investigation and artistic production, the development of a line of thought through its real phases, does not lend itself to testing by these means. The best way—in fact the only way—of seeing whether a person can do this sort of thing is simply to see whether he does it, *i.e.*, whether he can carry through investigation or produce beautiful works of art. Even much of the precision obtained in these tests is illusory. The false theory, already briefly discussed, that there are simple situations in which there is only one problem to be solved, recurs continually in the intelligence test, just as in the new type test we have the dogmatic assumption that there is only one right answer, and that this is definitely known to the examiner. There is no room for criticism.

Some of the examples given in Spearman's "The Nature of Intelligence and the Principles of Cognition" reveal an extraordinary dogmatism, desperately supported by a confused logic. Thus in the example

"cloth is to coat as leather is to . . ."

the answer is "footwear" and the solution "bag" is held to be quite wrong. "Clearly", says Spearman, "cloth is a fundament to the vestural character involved in coat" (p. 113). Using his own jargon, we could equally say that "cloth is a fundament to the enclosive character involved in coat". The relation between cloth and coat is not that of a fundament to a vestural character, but is simply that one is made out of the other. The fact that a coat is a piece of clothing, while it may lead us to say "footwear" in the last case (although equally this may be explained by saying that we are most accustomed to leather in footwear), is quite irrelevant to the ratio, just as irrelevant as the fact that a coat encloses the body in much the manner that a bag encloses goods. Things have various relations or associations, the association we select being determined by the forces operating in us, and their past experience. Some relations are more generally observed than others, but since there are always several relations existing objectively between things, it is quite arbitrary to settle on one of them as being the sort of association an intelligent person would make. Even where the terms associated by the subject have no objective connection, this provides evidence of mental

disturbance in the subject, but not necessarily of any incapacity to learn.

The truth of these observations is especially evident in the case of the "inventive" type of analogy, but in the selective type the same points have to be kept in mind. Things have various associations with other things, things "go together" in various ways. Certainly, in any inquiry it is important to select those associations which are relevant to the investigation in hand. However, we can only determine what is relevant in the light of past experience, so that again it is knowledge that is being tested. The plausibility of the view that we are testing capacity rests on a confused logical theory, in which relations are supposed to be imposed upon things, and a recognition that things are observed in situations, *i.e.*, with various associations, will destroy the notion of a special relating capacity. Further, the assumption is made that the problem under investigation is the one which the investigator wants to be solved. This is really what is meant by saying that the subject must be interested in the test, *i.e.*, he must really be interested in what the tester intended, and is not permitted to consider other relations between the materials presented.

The "opposites" test has special logical difficulties of its own, in that it reveals a misunderstanding of the nature of opposites. Two terms are opposites only when anything whatever is characterised by one or the other of them; but in mental tests terms which do not fulfil this condition, *e.g.*, good and bad, black and white, are treated as opposites. Here again the danger is greatest in the "inventive" tests. Thus Spearman claims that those who take careful to be the opposite of clumsy and cocksure of despondent are giving the wrong answers; and apparently he requires skilful and cheerful. But clumsy and skilful are no more opposites in a logical sense than are clumsy and careful; and if we take it to be merely a matter of exclusion, then again both pairs are in the same position. In the "selective" tests it is more evident what the tester requires, even though grave doubt may be felt about the accuracy of his views on the matter. The main point in regard to these "selective" tests is that the only way of making them difficult enough is to use uncommon words, so that the test comes down to a question of vocabulary achievement.

The tests, then, whatever else they are, cannot be described as tests of intelligence. At best, they show whether certain material commonly used in non-scholastic life has been successfully assimilated. They are quite incapable of determining what motives are concerned in the answering of the paper. A successful student must be "interested", but interest is not a motive but a relation, and whether the interest is the interest of curiosity, emulation, fear or various other activities in

different combinations cannot be determined by the measurement of success. And, again, the material of the tests may have been picked up under various circumstances ; and it is impossible to take these into account.

So far as the reliability of the tests is concerned, it may be observed first, that this reliability has been achieved by backwards methods, it being assumed that success in life is a criterion of intelligence, and, secondly, the environment which we have contended to have an important effect upon success in tests certainly has an important effect upon future success. While we have a constant environment we will expect to find a fairly constant "intelligence quotient". Further, too much reliance is placed upon averages. There is a tendency to treat recognised exceptions as being brought about by the operation of "factors other than intelligence", an amusing statement when one remembers that it has not been determined which factors are intelligent. While, then, a recognised exception is taken to show the incompleteness of the tests, it is not, as exceptions should be, taken to test the rule, to throw doubt upon the general operation of the tests.

Our conclusion is that whatever intelligence tests do, they do not test "intelligence" for the very good reason that there is no such faculty, and that their claim to predict with accuracy the future success of the student must be regarded with suspicion. At least we must interpose the very large condition "other things being equal". The bias of the tests is all towards the average orthodox student who can be relied upon to behave in an approved fashion. The claim that the present division between classes rests on a distinction in intellectual ability is vitiated by the demonstration that the successful answering of intelligence tests requires an environment of a particular character. Even mechanical tests will most easily be answered by those children who are accustomed to play with various forms of toys. Altogether, the tests require a very critical examination, and this examination can only proceed on the basis of a developed psychological theory, the establishment of which in turn depends upon the taking up of these questions philosophically. Psychologists undoubtedly have a special field of study, but they will make progress only by considering the logical implications of various hypotheses, not by desultory physiological experiments in the laboratory.

A STUDY IN PROBABILITY.

By D. TAYLOR.

I.

THERE is a move on the part of the hard-headed school of philosophers to deny the validity of all forms of reasoning except probability. At the same time a number of idealists are being tempted to seek support for a belief in values, from the recent assertions of scientists that there is an element of randomness in nature only to be stated in terms of probability. The following attempt to place probability upon a firmer base than the realist would think necessary or the idealist would allow is aimed against both views. For the first, an attempt will be made to show that probable reasoning involves both induction and analogy and frequently deduction as well. For the second, I do not believe that values are in any way safer in a random universe than in a determined one.

According to usage there are two sorts of probability, the one resting on evidence and the other on number. It is sometimes asserted that evidential probability could also be expressed as a numerical ratio if our knowledge were sufficiently minute, but most logicians think otherwise. Thus Cook Wilson: "It must, however, be insisted that the above (numerical ratio) is only one way of measuring the evidence, and is not applicable to all cases. Indeed, the more important matters of daily life usually do not admit of it, for there are qualitative differences in strength of evidence which cannot really be measured quantitatively, and that is why the application of mathematical probability to the testing of witnesses is so obviously futile."¹ It is the work of the first part of this paper to try to show that all probable judgments without exception are both based upon number and expressible (sometimes inaccurately) in terms of number. We shall therefore consider first the type of judgment of probability which is supposed to be neither numerically expressible nor numerically grounded.

In any judgment of this type there are a certain number of facts (the evidence) which the judgment must render intelligible, *i.e.*, with which it must be logically consistent. Further, there must be at least two possible explanations of the facts, since otherwise the one possible explanation would be necessarily true. All the facts known must fit each solution perfectly.

¹ Cook Wilson: *Statement and Inference*, § 327.

The smallest inconsistency between a known fact and a proposed solution will not merely render it unlikely, but will make it completely impossible. It is equally impossible that any fact should be *more* than consistent with a possible solution. There are no degrees of consistency either between inconsistency and consistency or beyond it. Thus the evidence for any particular conclusion which is not absolutely certain must be equally good evidence for at least one other conclusion. All that evidence can ever do is to make several conclusions individually possible and alternatively necessary. It cannot make one more likely than another. On the question of their relative likelihood it is completely silent.

A realisation of this fact has compelled many logicians to assert that, although objectively evidence may be only consistent or inconsistent with a conclusion, subjectively it may have a greater or less influence in producing in us a varying degree of conviction. "What we call the probability of an occurrence does not express anything objective, but the degree of confidence we entertain—a purely subjective matter."¹ Cook Wilson thus at once regards probable knowledge as lying somewhere between ignorance and knowledge, *i.e.*, as in some sense genuine knowledge, and at the same time as subjective. But surely knowledge cannot be subjective and remain knowledge. The Cambridge empiricists make a slight variation, asserting that probability lies between the certainty that S is P and the certainty that S is \bar{P} . And this view even more clearly infects the whole of knowledge with the supposed subjectivity of probability. If "I know S is P" is expressed $\frac{P}{H}=1$ and "I believe to a degree D that S is P" is expressed by $\frac{P}{H}=D$, then the subjectivity of the latter must attach to the former.

This general belief in the subjectivity of the judgment of probability arises, I think, from two sources. In the first place, there is the difficulty already referred to, that evidence cannot logically render one alternative more likely than another. This difficulty concerns the premises of the judgment, and we must return to it presently. In the second place, there is a confusion of opinion with probability which leads us to subjectify the conclusion of the judgment. In a statement of opinion I assert that "I believe S to be P", but admit in qualification, that I am fallible, that I may have overlooked certain evidence, and that the conclusion is not proven. Apart from these admissions, my belief is about fact, not about likelihood. But in a judgment of possibility it is assumed that there exists an objective relation-

¹ Cook Wilson : *Statement and Inference*, § 322.

ship between the possibility that S is P and the possibility that S is \bar{P} , and this relationship is made the ground of an assertion that SP is more, equally or less likely than $S\bar{P}$. There is no admission of fallibility, no suggestion that the evidence is incomplete, and the judgment concerns the relative likelihood of predicates, not the predicates themselves.

The reason for the denial of this distinction by the subjectivist lies ultimately in his conviction that objective reality can be neither likely nor unlikely but only possible or impossible. The form " S is probably P " lends colour to this difficulty. If this symbolism is correct, then the probability may attach to the judgment or to the relation between S and P and so to the object, but there is no other alternative. The necessity for a subjective theory is further sharpened, as we saw, by the belief that only varying degrees of subjective influence can distinguish, for us, alternatives which the evidence makes no more than possible.

Professor Stebbing, in her "*Modern Introduction to Logic*", says that we *naturally* assess the probability of an event E by the ratio of the number of reasons for the event to the number of reasons against plus the number of reasons for.¹ This account involves two suppositions, both of which, I think, are untrue. It implies that an event A , besides being consistent with E and with \bar{E} , may be *in favour of* E or \bar{E} . If this were not so, all the reasons for the event E would be as good reasons for \bar{E} , and *vice versa*. It implies, also, that the degree to which each fact favours E or \bar{E} is in all cases the same. If this were not so the probability of the proposition could not be stated as the ratio of the number of reasons for, to the number of reasons for and against. Even if the first supposition were correct, the second would be highly doubtful.

Mace seems to share Professor Stebbing's first assumption. "The relation between the proposition and the evidence is generally . . . analogous in some respects to the relation of entailment."² Entailment itself is impossible since that would remove the proposition from the realm of probability. Consistency is not enough. Here, too, a relation of favouring between the evidence and the proposition is assumed.

We are faced, therefore, with an alternative between a mystical view of favouring, and a subjective view of psychological impression, either of which would, in my opinion, destroy the validity of the probability judgment. Our position would be precarious if we could go no further. Evidence cannot establish differences of likelihood. Yet in fact we do make such distinc-

¹ L. Susan Stebbing: *A Modern Introduction to Logic*, p. 304.

² C. A. Mace: *The Principles of Logic*, p. 219.

tions, and, if these cannot be based on evidence, we must find a basis elsewhere or frankly abandon probable reasoning altogether.

Each conclusion is rendered possible by the evidence, but none is rendered necessary. It follows, therefore, that the facts given in the evidence are not the complete ground of any of the conclusions. (This is what the logicians quoted above meant by saying that the evidence in a judgment of probability is incomplete. It should be noticed, however, that it is evidence for certainty that is missing, not evidence for probability.) Expressed symbolically, if there are three possible conclusions, X, Y, and Z, the complete grounds of which are ABCDOS, ABC LMN and ABC QRS respectively, the evidence which makes each possible cannot be more than ABC, since, for example, ABCS would be inconsistent with Y. Let us suppose, then, that ABC are known facts. If we suppose X to be true, we shall have to postulate, in addition to the known facts ABC, the unknown facts DOS. If we suppose Y to be the conclusion, we must postulate LMN, and if Z, QRS. One of the solutions must be true, and therefore DOS or LMN or QRS must be fact. But which? It is plain that, if DOS is more likely than LMN or QRS, it follows that X is more likely than Y or Z and to the same extent. If, therefore, we can find a ground for asserting relative likelihood among what we may call our subsidiary assumptions, we have also grounded the original judgment. But it is clear at the outset that evidence will not serve as a ground.

Let us suppose two persons, one (A) facing us, and the other (B) turned away. A holds a bugle to his lips, but we cannot see whether B does so or not. We hear a sound which, let it be assumed, can be made only by one or the other of the persons. Either A or B may be responsible for it; both conclusions fit the facts perfectly though only one can be true. If A is playing, B, whose movements we cannot see, is not playing. If B is playing, A, who appears to be playing, is, in fact, not playing. We should commonly say that it is more likely that B, whom we cannot see, is not playing than that A, who appears to play, should not be playing, and if we could justify this claim we should be able to say that it is more likely that A is playing than B.

To proceed by Professor Stebbing's method, we should say that, while the holding of the bugle to his lips is a reason why we should think that A is playing, and the occurrence of the sound another, there is only one reason why we should think that B is playing. Hence there is a two to one probability that A is playing. But the holding of the bugle to his lips by A is as consistent with his not playing as with his playing. He may, for instance, be holding the bugle thus in order to be ready to

respond to B, and so his attitude is just as much a reason for a belief that the player is B.

Since, then, no piece of evidence can be said to favour one conclusion rather than another, since any subjective influence pertaining to evidence can only be a cause of and not a ground for the conclusion, there can be but one possible ground, and that the numerical relations into which the facts which constitute the evidence enter. The conclusion that A is probably playing must be based upon the fact that, in our experience, the number of buglers who appeared to be, and were in fact, playing has been great in proportion to those who appeared to play, but in fact did not play. I think there is no doubt that we do in practice base our judgment of probability upon some such experience of numerical proportions. If it can be shown that a valid inference to likelihood can be drawn from those proportions, the objectivity of both premises and conclusion is established. In the course of this examination it will be necessary to determine the meaning of an assertion of probability, bearing in mind the premises on which it is based. If that meaning avoids the contradiction involved in predicating likelihood of reality, a main reason why that objectivity was ever doubted will be removed.

II.

We concluded that because probability could not be derived from the qualitative nature of evidence it must be based on the numerical relations into which the facts which form the evidence enter. If this is the case, we shall have to consider (1) what a judgment of probability asserts, (2) how a knowledge of numerical proportion can justify a judgment of probability, and (3) how we may obtain a knowledge of numerical proportions.

Suppose a number N of red boxes and a number $4N$ of white ones. The boxes are wrapped in brown paper and made to pass in front of an observer who is required to assert of each box as it arrives whether it is white or red. In the first instance let it be assumed that four white boxes and one red are wrapped in regular succession, and let it be further assumed that the observer is aware of this without knowing what any particular box is. (He begins to observe the boxes after the procession has been in progress for some time.) The only way in which the observer can be certain of obtaining the greatest number possible of correct assertions will be to assert that all the boxes are white. If he follows this method he will be right on four occasions out of any successive five. This is true no matter where he begins or where he ends, provided that the total number of his assertions is either (a) made up of assertions about successive boxes, or (b) made up of groups, themselves assertions about successive boxes. The observer knows that each box is a member of a number of groups determinable in relation to it, of which the

assertion that all are white will produce one error in five. He may therefore say of each box that the likelihood of its being-correctly asserted to be white is $\frac{4}{5}$. And if all that this statement means is that, if any five or multiple of five boxes are asserted to be white, the degree of accuracy will be $\frac{4}{5}$, it cannot be denied that the judgment is justified. The ground of the decision rests, not on ignorance of what the boxes actually are, but on a thoroughgoing knowledge of the numerical order in which they are placed. This method of asserting all to be white though I know that $\frac{1}{5}$ are not white is the basic principle of probability. Its logical justification depends on this, that we can, through it, obtain a greater measure of accuracy than through any other method, and the degree of that accuracy can be calculated exactly.

Broad bases probability on an axiom of equi-probability. If of n possibilities nothing is known why one should be actual rather than another, they are equi-probable. This view has the demerit of making ignorance a ground of knowledge. It might be argued, too, that it offends against the principle that the real cannot be likely. Assuming the objectivity of probability, equi-probability can be asserted of two mutually exclusive possibilities only if it is given some such meaning as was suggested above. Even apart from these difficulties there seems no reason why it should be regarded as more evident that a box known to be red or white should be *equally likely* to be red, rather than twice or three times as likely. It is in fact quite common to discover regular proportions which are not 1 : 1.

In the second instance we shall suppose that, although the boxes are not wrapped regularly, the amount of divergence from regularity is small. Here the observer knows what error will be made if all the boxes are declared white, but he does not know the proportion of error in asserting *any* particular number of boxes to be white. Hence he cannot say that, if he acts as though the boxes are known to be white he will be right four times out of five, since the ratio is general. And so it is not true that the probability that *any* box is white is $\frac{4}{5}$.

If, however, the observer knows that the amount of divergence from the ratio $N : 4N$ is never more than $+a$ or $-a$, he can calculate within those limits the number of correct assertions in any given group on the hypothesis that all are white, and so can tell whether such an hypothesis is a satisfactory basis for action. Therefore he can judge that the probability

of any box being white lies between $\frac{4N-a}{5N}$ and $\frac{4N+a}{5N}$.

Moreover, if he knows, beside the amount of divergence, the frequency of alternation of positive and negative values, he will be able to determine what size of group must be taken in order to cancel out the positive and negative divergences, but this, while it will add to the groups of which he knows the proportion, will not make that proportion general, and so will not affect the probability of any particular box.

The cases we have been considering are all of closed classes, but what has been said of them applies also to open classes. Suppose a coin spun in such a way that in the first three falls two are tails and one a head, in that order, and that this order continues indefinitely. An observer knowing this, but without knowing the character of any particular fall, will be able to calculate exactly the error involved in a declaration that any group of successive falls is all tails. He can therefore gauge the probability of the coin falling tails to be $\frac{2}{3}$. If, now, instead of each group having the same ratio of heads and tails, there is only an approximate similarity, it will be possible for the observer to estimate within the limits of divergence the amount of error involved in the declaration that any group is tails, and therefore the probability of any toss being tails. But if the divergence is too great, the probability will be valueless. No judgment of probability can be made where there is no approximation of groups within a class to a given ratio.

From the foregoing we may state that the probability of any Z being Q may be determined where all Z's are a class in which any group has the same proportion of ZQ's and ZQ's as any other group. The class of boxes that are wrapped in regular succession four white and one red is such a class. The class of tossings of a coin in which tail and head succeed each other (approximately) in the ratio of 1:1 is such a class. A class in which no such approximation between groups exists can give no ground for a judgment of probability. A class which is so divisible I shall call a uniform class.

So far the existence of such classes has been assumed, but we must now consider how the uniformity of a class can be determined. The possibilities are (1) by counting, (2) by deduction from the nature of the class, (3) by analogy.

The determination of a class by counting may be dismissed at once. It is of use only in problems such as the one of the coloured boxes. The question of uniformity is bound up with that of random selection, but there are two reasons why I prefer the more unusual terminology. (1) The notion of random selection suggests the prior existence of a class exhibiting certain

proportions from which the selection is made, and although there are classes like this, in more cases the method of selection creates the class. (2) The word "random" suggests haphazard selection, whereas, in fact, a method which will produce random results is the reverse of haphazard. We must consider first what it is that we want when we make a random selection. In view of the fact that we assert of the selection characteristics known to belong to the whole class, it may be argued that, even if we cannot define a random sample as one exhibiting the same proportions as the class, such a statement offers an ideal of what random selection should produce, and divergence from it must be considered as implying a defect in the method of selection. This is not strictly accurate. All that is necessary to obtain a random sample is that the relation between the proportions of the sample and the proportions of the whole class should be regular. It is not necessary that they should be the same. It appears, therefore, that, far from seeking a method of selection totally unconnected with the characters to be selected from, we must choose a method that is regularly related to them (numerically). Such regular numerical relation can only rest on real connection.

It might have seemed obvious that if there were regular numerical relationships between a class and samples taken from that class there must be causes of that regularity. But, in fact, the opposite has been contended. Keynes argues that, if there is no causal connection between the causes of two classes a and b where b is a part of a , a and b will both exhibit the same ratios of, say, Z and \bar{Z} . That is, he rules out the possibility that such a method of selection may be either uniformly biased or merely erratic. In the case he gives the class is the class of the numbers of inhabitants of towns, and he contends that the uniformity of this class can be inferred from the lack of connection between a number's being odd and its being the number of the inhabitants of a town.¹ But it can be shown, on the contrary, that the uniformity of the class rests on necessary connection. The factors which make the population of a town what it is are the numbers of births, deaths and migrations, and each of these numbers, we know from experience, lies in a uniform series 1, 2, 3, 4, *et cetera*, in which odd and even numbers are equal. It follows, therefore, that the number of inhabitants is also a member of such a series, and so has an even chance of being odd. If birth were always of twins, if, whenever a man died, his closest relative were buried with him, and if migration were forbidden, the number of inhabitants would not be as likely to be odd as even. It would necessarily be even (assuming this state of affairs to have existed from the town's beginning).

¹ J. M. Keynes: *A Treatise on Probability*, p. 291.

In discussing randomness in general, we have disposed of determination of uniformity by deduction, so that there remains only its determination by analogy. If we have found from experience that a certain method of forming a class has on occasion produced a uniform class, we may argue that this method will always do so, provided that there is no relevant difference in the circumstances of its use.

The argument from analogy involves two important assumptions: (1) that there are qualitative causes of the numerical uniformity of a class, (2) that without knowing the actual causes of such uniformity, we may know that certain events or characteristics are not part of them. That the first assumption is warranted is hardly doubtful. Chemistry offers us the most obvious examples (water is decomposed by electrolysis into two of hydrogen and one of oxygen). And the second will not be questioned by anyone who reflects that the rejection of numerous possible causes is usually the first step in scientific enquiry.

The argument from analogy is easily the most important method of determining a uniform class. Most of our judgments of probability are concerned with the final members of unclosed classes whose uniformity cannot possibly be verified by inspection. And it is only infrequently that the causes of the ratio are known and consequently that the deductive method can be used.

This account of the method of probable reasoning has necessarily been rather sketchy, but it will have been a success from the point of view of its writer if it raises doubts of the truth of a view of probability which makes a great deal of the normal thought processes of scientists and ordinary men invalid.

RESEARCHES AND REPORTS.

AN ELECTRICALLY RECORDING DOTTING MACHINE.¹

By HENRY H. FERGUSON.

I. GENERAL.

THE apparatus to be described is a descendant of McDougall's dotting machine in spite of its marked difference, and a final product of various experimental models. The same type of aperture is used for presenting the "dots" as was used by McDougall, and use is made of a pattern almost identical with his. In the present apparatus, however, the number of hits is automatically counted by making the "dots" of metal and by connecting each of them with an electrical circuit, which is completed when they are struck and which operates an electrical counter.

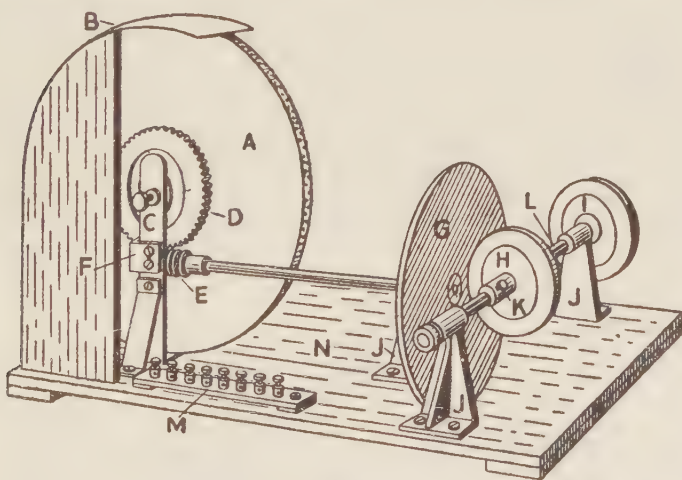


FIG. I.

A. Wooden Drum.
B. Aperture.
C. Drum Supports.
D. Worm Wheel.
E. Worm.
F. Worm Bearing.
G. Friction Disc.

H. Friction Drive Wheel.
I. Pulley.
J. Bearing Brackets.
K. Friction Wheel Locking Screw.
L. Key-way in Drive Shaft.
M. Terminal Block.
N. Base Board.

¹ Made to the author's specifications by G. S. Marshall, Scientific Instrument Maker, Dunedin, N.Z.

The present apparatus consists essentially of the following parts : (i) a revolving metal band whose outer surface is covered with insulating material in which has been punched a number of circular holes, (ii) a contact stylus by means of which the subject attempts to hit the metal as it shows through the holes, and (iii) two Veeder electrical counters. These parts are so arranged that one counter records all hits, while the other records all attempts, whether hits or misses. With the band revolving at a known speed and, in consequence, presenting a given number of "dots" per minute, it becomes a matter of simple calculation to tell the total number of "dots" presented in a given time. Hence for any given time, with the band revolving at a given speed, it is possible by means of the present apparatus to ascertain easily (i) the total number of "dots" presented, (ii) the total number of attempts by the subject, and (iii) the total number of hits by the subject.

The general layout of the apparatus is given in Fig. I.

II. DRUM (Figs. I, II and III).

The "dots" are presented by means of a revolving drum rotating on conical pivot bearings. It has a diameter over all of 33 cms., and a width over all of 3 cms. The main body of the drum is of wood (which must be well seasoned). This part is made with a slight flange on one side to allow the monel metal band (the contact strip) to be forced on. The perforated celluloid band consists of a length of cinematograph film painted over, to make it non-transparent, with black cellulose lacquer on the under surface. In making the perforations, 209 in number, a strip of McDougall's dotting machine paper was fastened over a length of film and the holes punched according to the pattern

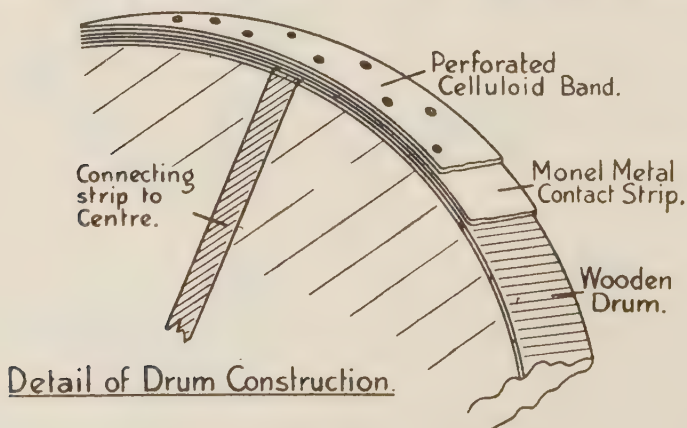


FIG. II.

and size used by him. Celluloid cement was used to transform the perforated celluloid strip into the necessary band, which was made slightly too tight, expanded with heat, and slipped over the metal band. The metal and celluloid bands are 2.6 cms. wide. A celluloid rather than a paper band is used for the following reasons: (i) It wears better, (ii) it can be made to fit better without being made to adhere to the metal band, and, consequently, (iii) can be slid round on the metal band so as to expose fresh metal surfaces, when the others become oxidised and (iv) can be easily removed.

The drum is revolved by means of a worm drive (see Fig. I), which ensures a steady rotation at the low speed used (normally only a little over one revolution per minute) when the cam engages with the Veeder counter (see Fig. III) used to determine

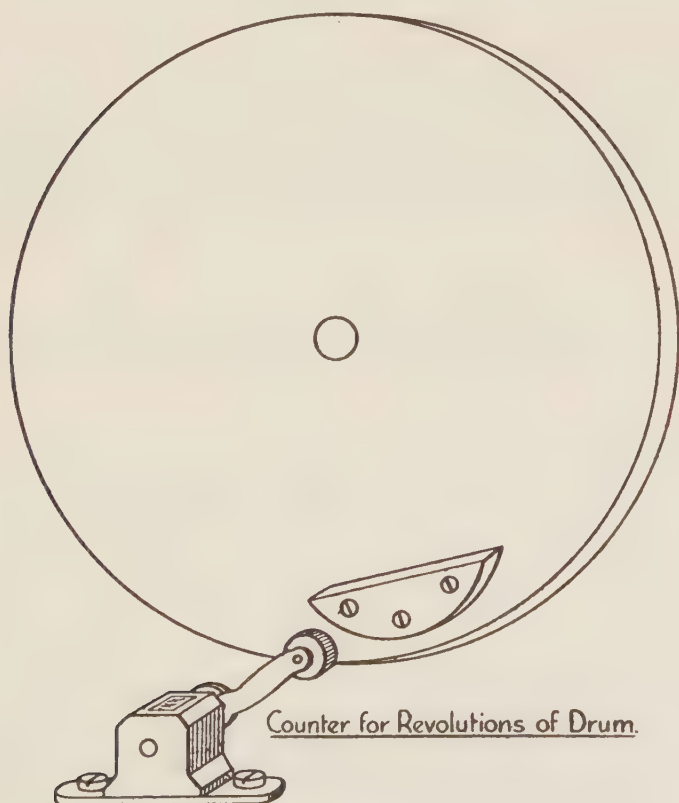


FIG. III.

the number of revolutions made by the drum. In an earlier model a belt drive proved unsatisfactory in that the drum tended to hesitate, when the cam engaged with the counter. A narrow metal strip welded to the contact strip connects it with the metal at the centre of the drum.

III. APERTURE.

To present the "dots", the same type of aperture is used as was employed by McDougall—2·7 ins. wide by 2·6 ins. high, when fully opened, and operated by a screw adjustment. In the present model the base of the aperture is only 1 mm. from the celluloid band, and is kept approximately at that distance by a strong wooden framework to which the metal containing the aperture is attached. With the "dots" appearing close to the aperture, it is easier to keep them clearly illuminated.

IV. DRIVING GEAR (Fig. IV).

This consists of a friction drive wheel, a friction disc (metal), worm and worm wheel. The friction drive wheel, which has a diameter of 10·2 cms. over all, is fitted with a round-shaped solid-rubber tyre, which makes a contact of about 3 mms. with the friction disc. This friction drive wheel can be moved along its drive shaft and so brought into contact with different parts of the friction disc. By this means a large variation in

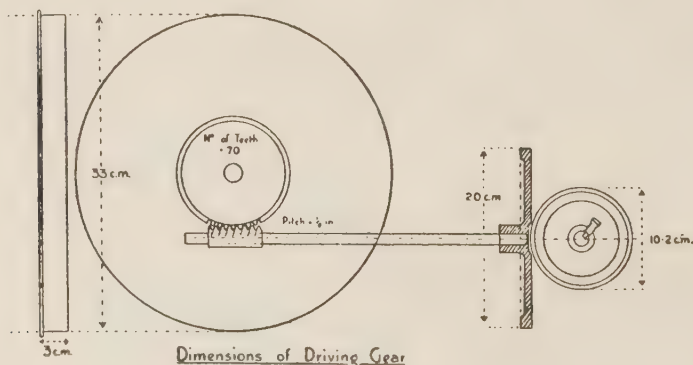


FIG. IV.

the speed of the friction disc can be obtained, with consequent large variations in the speed of the drum and the presentation of the "dots". To provide a means of preventing the flattening of the rubber tyre, which would arise from its constant pressure against the friction disc, when the apparatus is not in use, a depression, 4 cms. in diameter, is made in the centre of the

friction disc. When the apparatus is not in use, the friction drive wheel is moved opposite this depression and so disengaged from the friction disc.

It is possible to bring the friction drive wheel into effective contact with the friction disc over 7 cms. of its radius. With the friction drive wheel making 32 r.p.m., that means a possible variation in the speed of presentation of the "dots" of from approximately 209 to 53 per minute. With 47 r.p.m. it means a possible variation of from approximately 304 to 78. Since it would appear that a presentation of about 115 "dots" per minute is as many as the normal adult can cope with for one or two minutes, and more than he can cope with over a longer period, it appears that with the friction drive wheel doing 32 r.p.m., a sufficient variation in the speed of presentation of the "dots" could be obtained for most purposes.

The worm and wheel are made to $\frac{1}{8}$ in. circular pitch, the worm being a single thread screw and the wheel having 70 teeth.

V. CONTACT STYLUS (Fig. V).

"Contact stylus" is the name given by the author to his modification of a contact pencil.¹ This stylus has a length over all of 15 cms., a diameter of 0.8 cm., a point 1.3 cms. long by 0.05 in. thick. The whole stylus is made of metal

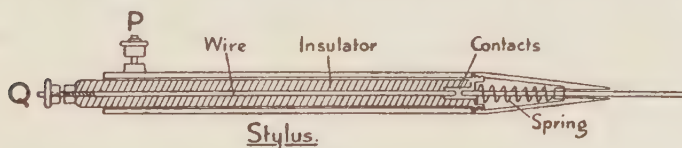


FIG. V.

except for the insulating material, in which are set the terminal Q and the wire attached. Pressure on the point of the stylus causes it to overcome the resistance of the spring and move back into the body of the stylus. When this happens, two platinum contacts in the interior of the stylus are brought together. And, since the metal point of the stylus is constantly in contact with the metal casing of the whole stylus, an electrical circuit attached to the terminals P and Q may be completed every time the point of the stylus is moved back and the platinum contacts brought together. It is this circuit which operates electrical counter A (see Figs. VI and VII), which records the number of attempts, successful or otherwise, made by the subject. The point of the stylus and the metal casing are made

¹ Supplied by Hawksley & Sons, 83 Wigmore Street, London.

to form part of the other circuit which, leaving from terminal P and proceeding to the metal contact band, is completed each time the point of the stylus is brought into contact with the band. It is this circuit which operates electrical counter B which records the hits.

Only a small distance (1 mm.) exists between the two platinum contact points. This has been found necessary. If the distance is larger there is the possibility of the subject actually making a number of attempts without these being recorded, due to the fact that the stylus point may not have moved sufficiently to bring the contact points together and complete the circuit. Even as it is, the method of recording is not absolutely fool-proof. For example, if the subject were to *draw* his stylus over the surface of the drum, only hits would be recorded. Such a practice, if it did occur to any extent, would be easily detected. With the co-operation of the subject, however, the method of counting appears reliable.

To avoid obstructing the subject's movement, light flex is attached to the stylus.

VI. CIRCUIT (Figs. VI and VII).

When the principle of the contact stylus is grasped, apprehension of the circuit becomes quite simple. Every time the contact points within the stylus are brought together, and that occurs with every attempt, one circuit is completed and one

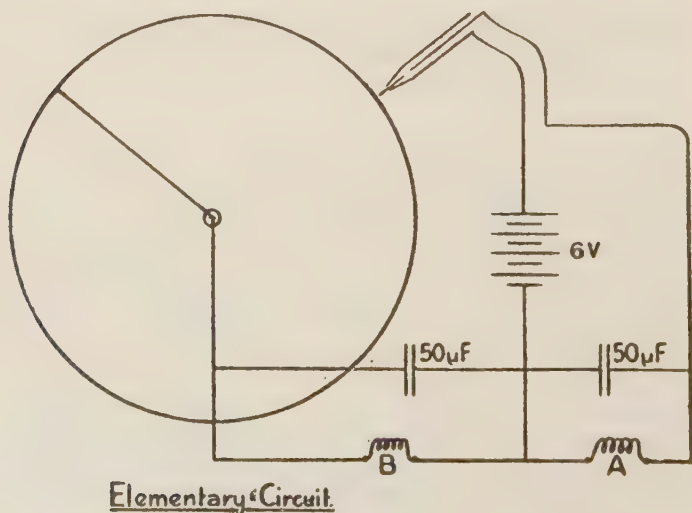


FIG. VI.

electrical counter (A in diagrams) is operated. Every time a hit is made the other circuit through the monel metal strip is completed and the other electrical counter (B in diagrams) is operated also.

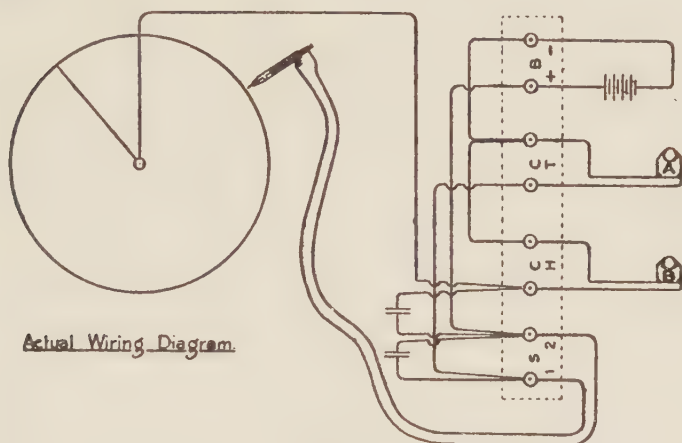


FIG. VII.

Two Veeder electrical counters requiring a 6-volt battery for their operation are used. With this amount of current the size of the spark emitted every time a hit was made was, at first, considerable, with the consequent possibility of setting fire to the celluloid, and the rapid oxidation of the monel metal. To reduce the size of the spark, two condensers of the electrolytic type, each of 50 microfarads capacity, are now introduced.

REVIEWS.

DIE PHILOSOPHISCHEN STRÖMUNGEN DER GEGENWART IN GROSSBRITANNIEN. By Rudolf Metz. Leipzig: Meiner.

The publication of this work by Rudolf Metz is an event of importance, both in England and in Germany. For a long time German thinkers have been contented with a rigid and unhistorical picture of British thought: England was the land of naturalism and empiricism and nothing else, and Mill and Spencer were its latest and most typical representatives. In recent times the writings of Russell have created a considerable stir in Germany, but there has been little knowledge of the background of Russell's thought, and little capacity to separate

his personal extravagances from the philosophical tendency he represents. The whole neo-Hegelian movement in British thought, which preceded the rise of realism, was utterly disregarded until Herr Metz began to call attention to it in various philosophical periodicals. At the same time, British thought has not been sufficiently self-conscious to produce a history of its own development, and must therefore derive great profit from a record of its achievements, from Reid up to the present day, which is at once accurate, impartial and exhaustive.

Herr Metz has written his book more from an historical and psychological than from a logical point of view. He appears to be less interested in the truth of doctrines or their logical coherence than in the fundamental attitude to reality which they represent. He believes that a piecemeal empiricism, which deals with isolated problems in preference to constructing speculative syntheses, is the natural attitude of the British mind. British philosophy, he thinks, was temporarily diverted from this attitude by the influx of Hegelian ideas in the latter part of the nineteenth century, only to return to it more vigorously in the ensuing period. Herr Metz himself adheres to the classical German tradition, and believes, as British thinkers no longer believe, that Kant and Hegel are the culmination of Western thought, but he seems to find it refreshing that other points of view still manage to maintain themselves.

The first part of the book gives a masterly sketch of British empiricism and naturalism in the nineteenth century. The many strident voices of Victorian enlightenment resolve themselves into a few simple utterances, which are now mainly of historical interest. Herr Metz has read practically everything, including works which have fallen into deserved or undeserved neglect. He shows extraordinary sympathy towards thinkers like Mill, with whose main positions he cannot possibly agree. His treatment of Sidgwick and Spencer is, however, a little less satisfactory than the rest of this part of the work. As regards Sidgwick, Herr Metz seems to regard exactness in philosophy as astonishing rather than useful, and the epoch-making character of *The Methods of Ethics* is not fully appreciated. In the case of Spencer, he fails to realise that evolution was to a large extent an afterthought, and that Spencer's essential positions are to be found in such an early work as *Social Statics*. Herr Metz has given admirable sketches of the thought of Newman, Martineau and Hobhouse, who are far too little regarded at present.

In the second part of the first volume we have a sketch of the Hegelian movement, which appeals strongly to Herr Metz as a fulfilment of "the world-historical mission of German idealism". The development of the movement is described in rather a lively manner: we see empirical England gradually conquered by an idealistic attack which originates in Oxford

and the Scottish universities. Herr Metz recognises to the full the great originality and genius of Bradley and Bosanquet, and the extent to which they clarified and developed the logical bases of Hegelianism. McTaggart is dealt with among the Hegelians, though it would have been equally possible to classify him among the neo-realists. In this case, too, we come up against Herr Metz's dislike for philosophical exactitude: he is more deeply impressed by McTaggart's mystical conclusions, which are the common property of all grades of thinkers, than by the rigour of his arguments.

In his second volume Herr Metz devotes a short section to the pragmatism of Schiller, and then deals mainly with the older and newer forms of realism. His careful treatment of the forgotten writings of Shadworth Hodgson is very interesting: he shows that many of Hodgson's analyses anticipate the work of the German phenomenologists. In his treatment of realism Herr Metz is accurate and impartial, but shows an inability to understand the fundamental arguments on which realism is based. The notion that thought can be in immediate contact with objects other than itself seems to him a wanton paradox, interesting only as a revelation of British hard-headedness and naïveté. No finer proof of the importance of modern British realism can be given than the profoundly puzzled but generous admiration with which Herr Metz records the philosophical achievements of such thinkers as Cook Wilson, Moore, Russell and Broad. It is a great pleasure to find Cook Wilson worthily characterised as a British Socrates. In the case of the Cambridge thinkers the only important criticism is that Herr Metz has not paid sufficient attention to the early logical realism of Russell in such treatises as *The Principles of Mathematics* or *The Philosophy of Leibniz*. He is not interested in such topics as propositions, facts, universals and relations, or in the treatment of the problem of internal and external relations. These contributions of realism are far more important than the realistic theories of perception which are analysed *in extenso*. Herr Metz's omission is probably due to his distaste for modern logic, which he regards as something merely technical and mathematical, instead of as a method which transforms every problem to which it is applied. Herr Metz finds the unmetaphysical and problematic orientation of modern English thought very bewildering: Broad, who is treated as a new psychological type, is censured for refusing to make up his mind between alternative theories, as if it were a virtue to make up one's mind on insufficient evidence. The speculative syntheses of Alexander and Whitehead are more to the author's taste.

Herr Metz concludes his book with an interesting treatment of British psychology, scientific philosophy and philosophy of religion. Stout and McDougall are treated as the most important

British psychologists, and the acuteness of Stout's analyses is properly appreciated. Among the scientific philosophers Jeans, Haldane and others are briefly mentioned, and the concluding section deals with such religiously-minded thinkers as Dean Inge and Baron von Hügel. Herr Metz's book may be imperfect as a study of philosophical theories, treated as logical structures, but it is a wholly admirable study of these theories as historical and psychological phenomena and expressions of a culture which refuses to analyse itself.

J. N. FINDLAY.

THE ARGUMENT OF PLATO. By F. H. Anderson, M.A., Ph.D., Associate Professor of Philosophy in the University of Toronto. Dent. 10/6 net.

The object of this work is to present the essential Plato who exists through and above controversy and chronology, and its method is to recapitulate the more important dialogues, and to show how the "argument" proceeds continuously from one to another. Given the original intention, it has certain outstanding merits. It is excellently written; the translation of the substantial and numerous extracts is both careful and euphonious; and the arrangement of topics, which proceeds by a continuously deepening dialectic from the more immediate to the more fundamental problems, is clear and effective. It displays a close acquaintance with the text, and is marred by no important omissions.

But it may be doubted whether the intention was really worth the care bestowed on its execution. In the first place, it excludes any original contribution to Platonic scholarship. It is possible to read from cover to cover without stumbling on any original insight, or any serious attempt at a critical estimate. There is one exception, and that is the defence of Plato's view of poetry, in Chapter XIII. It is not a successful excursion, for the author takes refuge in the equivalence of goodness and beauty, and does not face the crucial question: if "rhythm and harmony" are "sisters to good words and good nature", why censor the poet? In general, however, the interpretations he adopts are extremely conventional, and it is to be feared that the mellifluous *legato* of his writing is incompatible both with the unexpectedness of genius, and with the harsh duties of critical investigation.

A further objection to the author's plan of attack is that his book will be particularly serviceable to the undesirable type of reader who wants to find out what a philosopher has to say without having to read him. Such parasites do not deserve the trouble he has taken for them, but the fact that he has taken it may mislead them into supposing that they are worthy of

attention. For the intelligent student of the dialogues there is little that is new, though the pursuit of the conceptions of order and measure from one dialogue to another may be useful to those who may have been tempted to think of them in isolation.

There is a further assumption behind the author's objective which seems to me to be unacceptable, and that is that the successive works of Plato form a continuous exposition of a doctrine held firmly from the beginning. This assumption is avowed on pp. 204-205, where he suggests that the discussion of pleasure is "deferred" from the *Protagoras*, universally held to be an early dialogue, to the *Philebus*, which is equally certainly a late one, till the doctrine of "irreducible measure", on which the solution of the *Philebus* depends, should have been expounded. I believe the assumption to be untrue, both in its general and in its particular form. It is incompatible with the positive reversal (inadequately described as development) of certain doctrines of the earlier dialogues (e.g., on not-being, and on the status of Forms) in the later and maturer ones. Consequently I hold that the proper way to study Plato is to get his writings arranged chronologically by the stylistic experts and to approach them in their historical order. In this connexion, however, it is fair to record that the author's order of topics does roughly correspond to Plato's order of development, and that he recognises (somewhat inconsistently) the opposition between the earlier and the later handling of important topics.

It will be seen that the value of the work will depend on the estimate of its objective. Those who are interested in the progress of Platonic investigation will find it unnecessary, though, in view of the attractive presentation, it is never tedious. Those who prefer their philosophy elegantly prepared for facile consumption will find it a godsend. In any case, within what I venture to consider its regrettable limits, it is both persuasive and efficient.

A. BOYCE GIBSON.

POLITICAL ETHICS. By D. S. Robinson. Thomas Y. Crowell Company, New York. 1935. Pp. 304. Price 2 dollars.

Professor D. S. Robinson commences his study of Political Ethics with two complaints. Of these the first—that political theory has been divorced from philosophy—will win considerable sympathy, for in Australia, too, we have had experience of "courses of government" which "slur over the basal issues of political philosophy, and emphasise excessively the minutiae of political organisations and their functioning" (p. xiv). His second grievance—that ethics has been divorced from politics—

is much more open to question. In fact, Professor Robinson's own book serves as an excellent example of the confusion which results from the intermingling of these two studies. Even if we grant that political forces can have ethical characters, this does not mean that there is any question of "applying" ethics to politics or of "political ethics". Mental operations certainly have ethical characters, but we do not therefore conclude that we have to apply ethics to psychology, to develop a special "psychological ethics", nor would we say that the separation of psychology and ethics can only be detrimental to both sciences.

Of course, like most ethical theorists, Professor Robinson speaks in terms of ethical qualities, but is really concerned with relations. Thus his principal theme is the relations into which States may rightly enter with other States, professional groupings, "protected" peoples and their own nationals. As a necessary preliminary to the solution of this problem, he examines the nature of the State and, more especially, the nature of the Ideal State. Here he speaks of good (although he prefers "value") and evil, but his primary concern is still with relations. He takes as "Three Kinds of Evils Against Which Man Contends", first, "The Clash of Ideals", secondly, "The Competitive Nature of Economic Value", and thirdly, "The Fixity of Institutions". Removing pretentiousness, these three evils appear in the following form: that different people want different things, that the one thing can only be possessed by one person, and that institutions tend to retain their original characters. Now, the only circumstance which appears to be common to all these three is that they hinder the establishment of the type of Ideal State envisaged by Professor Robinson. This hindering is nothing against their being evil, for although we cannot *define* evil as "that which hinders", it is nevertheless true that evils *do* hinder goods; but we should expect to find some characters indicated, as distinct from mere reference to the *persistence* of certain institutions or the fact that certain objects are *competed for*. If we are to establish a positive theory of evils, we shall require something more than these vague references. What we commonly describe as evil, in fact, is not the relations which objects have to persons, but certain mental operations, *e.g.*, lust for possession or avarice; and in the same way we regard certain desires as evil and similarly we might talk of obscurantism, the suppression of criticism by vested interests, as an evil. Professor Robinson, then, may be referring to certain evils in a vague sort of way, but as his theory is stated we can only say that he simply assumes the identification of evil with the hindering of good.

This aspect of the matter, Professor Robinson's tendency to identify the good with the wanted and the evil with the

opposed, appears clearly when he formulates his postulates of an Ideal State, and attempts to apply these postulates to various communities, the Soviet State, Dictatorships and Representative Democracies. These postulates are quite literally "things demanded", and it is not shown that there is anything ethical about them. If the postulates are the conditions under which goods alone can operate, then this can only be made clear by a precise consideration of goods and their conditions. This discovery would only have political significance for those who wanted goods, and its interest for the ethical scientist would only be incidental.

Ethically, Professor Robinson is nothing if not broad-minded. He shows a special interest in Kantian doctrines, placing great emphasis upon the treatment of humanity as an end, or, as he puts it, the doctrine that "personality should be put above property" (p. 6), and regarding the knowledge of ethical principles as an essential prerequisite of ethical activity. Further, we find an emphasis on "good will", but we are informed that Kant has a "tendency towards a purely formalistic theory of ethics" (p. 6) and certainly nobody could accuse Professor Robinson's ethical theories of being formal. Thus, he everywhere makes ethical statements of a dogmatic character, but, nevertheless, "morality is largely a personal matter" (p. 207). Elsewhere, we learn at one time "that there is a latent idea of good in each human being, a unique moral deposit that guides him in his quest for full self-development" (p. 47); at other times we hear of "the principle of moral symmetry" (p. 226) and discover "that those who are conscious of receiving benefits from an institution have a right to fight for its preservation" (p. 57), while, further, the cultural order has "a rightful place of superiority" (p. 75) and business must be kept "in its place" (p. 144).

The ethical scepticism, the sheer inability to think of ethics as a science, which underlies this hotch-potch of Intuitionism, Legalism, Utilitarianism and Functionalism, appears clearly in statements like the following on the subject of forgiveness and non-resistance: "These virtues are possible for individuals and they are truly ethical when they are so practised as to make the person toward whom they are directed change his ways. For when they are so practised they generate in others the attitude of forgiveness and of non-resistance. States cannot practise good will in this extreme form. Every State must resist invasions from other States" (p. 167).

This is the dogmatism of scepticism, the mere laying down of the law without regard for consistency. Professor Robinson has in fact abandoned the two really basic points made by Kant—first, that if an activity is good or evil, it is so independently of results and conditions, and secondly, that if

an activity is good, it is so quite generally or universally, the two points, of course, being closely bound up together. Only if this is true can there possibly be anything describable as ethics, anything more than the mere formulation of demands.

Professor Robinson's method is simple in the extreme, consisting in the application of whatever "ethical principle" suits his purpose at the moment or, where no ethical principle is at hand, the reliance on his own "idea of good" as a test of ethical quality. Professor Robinson is not, however, unique except in his extreme eclecticism. Ethical theorists who concern themselves with obligation, with "what is to be done", quite generally lay down a principle or criterion, the vagueness of which prevents concrete application, and then go sublimely ahead to formulate their own demands, appealing to the demands of their readers for confirmation.

Thus we find ourselves inclined to accept Professor Robinson's "shoulds" whenever they coincide with our own political opinions, and to reject them when we have different views on the matter. But there is nothing ethical about this, and the intrusion of a pseudo-ethical element, while it assists Professor Robinson's tendency to seek an easy solution, his "ethical optimism", can only hinder a clear recognition of the political issues. If Professor Robinson's scepticism is carried out to its logical conclusion, then there is nothing but politics, and ethics is only a confusion; while if on the other hand there is a positive ethics, then goodness exists independently of our demands, and must be clearly separated from politics.

This condemnation of Professor Robinson's main thesis and the continual inconsistencies in his theory, does not imply that there is nothing of value in his work. On the contrary, he quite frequently brings out the political issues clearly, this being especially evident in his treatment of economic imperialism. Nevertheless, his concern with judging often obscures the issues themselves; and he unduly simplifies a great many questions.

Similarly, in the ethical sphere, his mainly relativist approach does not prevent him from making several interesting points, particularly in his treatment of "The Ideal State", where his temporary concern with goodness, as distinct from rectitude, makes positive ethical theory possible. For example, he clearly distinguishes economic and ethical goods, on the ground that the latter increase with sharing, while his emphasis on "the cultural order" reveals a mind not quite overcome by mere moralism. What we require here is a rejection of the distinction between the three orders, private, public and cultural, and a fuller recognition of the point, made to some degree in Professor Robinson's assertion that a man is never successful in one of these fields alone, that goods work together in a special way. We simply have positive goods—human

affection, courage and the spirit of production, the love of beauty and the love of truth, and any of these are possible in private life, social activity, or the world of art. Professor Robinson takes as "orders" what are really distinctions in character. With an insistence on the theory of positive goodness as the subject matter of ethics, and the recognition that "rectitude" is simply what is demanded by some group, the confusion of ethics with politics is less plausible. It might then be possible to develop a genuine ethics on the one hand, by rejecting from it all traces of obligation, and a genuine sociology on the other hand, by removing from it all traces of pseudo-ethics.

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PHILOSOPHICAL STUDIES. By J. McT. E. McTaggart. Edited by S. V. Keeling. Pp. 292. London: Edward Arnold. 1934. 12/6 net.

Of the eleven essays collected here, all but one (a synopsis of one of McTaggart's lecture courses) have previously been published. Two ("Dare to be Wise" and "Mysticism") are taken from rather inaccessible sources; the remainder originally appeared in well-known periodicals and books. The most important of them were embodied by McTaggart in his other works. Thus the present volume does not add greatly to a knowledge of McTaggart's general position. At the same time, it should be mentioned that these studies present an excellent survey of his intricate philosophical system. Most of the main doctrines of *The Nature of Existence* appear here, and Dr. Keeling supplements the text by frequent cross-references to that and other of McTaggart's more important works.

In his introduction, Dr. Keeling discusses Russell's attack on metaphysics and shows the way in which McTaggart would answer it. McTaggart's own view of the relation between science and metaphysics is stated in these papers. It is Russell's complaint that most metaphysical systems are attempts to give "legislative force to our wishes"; and McTaggart is not afraid to avow the connection between his wishes and beliefs. "Science", he says, "is not interested in the ultimate nature of reality in the subjects it deals with, but only with what is—comparatively—on the surface . . . The utility of metaphysics is to be found in the comfort it can give us . . . If we go farther [than science goes] we may succeed in arriving at a belief in Idealism and that gives us a much more cheerful view of the universe." As McTaggart represents the search for "a cheerful view of things", it appears that success is dependent upon two main conditions: upon our being able to establish the unreality of time and to demonstrate a particular view of the relation

between time and eternity. Other mystical systems have been content merely with the former doctrine; McTaggart refers to Hegel, who held, he says, that since time is unreal, and the eternal alone real, evil and sin are also mere illusions. McTaggart goes beyond this, and incidentally sets himself an impossible task. He agrees that time is unreal, but the appearance of things as being evil and in time is real, and this itself is a real evil. The question arises here whether this is not a repudiation of the first principle, the unreality of time; for we may argue that if things really do appear to be in succession, that is possible only because they appear successively. However, McTaggart's position is that the eternal appears to us as in time and imperfect; but a time will come when eternity does not appear in time at all. As he puts it, "Time [is] essentially the process by which we reach to the Eternal and its perfection". Thus, he sets himself to reconcile contradictions. The non-temporal is logically ultimate (*i.e.*, alone real), but it is also temporally ultimate—the final term in the time series. It appears that time is both real and unreal, and this is connected with a further contradiction, that what appears as in time is the eternal, while, on the other hand, the eternal is really to be found only at the point where the time series ends. In this way, McTaggart attempts to take account of the two points, that events are not temporal, since time is unreal, and also that they are not non-temporal, *i.e.*, not the eternal.

In essay VI, "The Relation of Time to Eternity", McTaggart contends although some things appear as temporal, in reality there is only the eternal. But while events have no real temporal order, they are ordered, and he suggests that the order of terms of the series of events is determined by the degree of adequacy with which each represents the eternal reality. Thus time is an appearance of something else: present, past and future can be only degrees of approximation to the eternal. But as the argument develops, it becomes clear that this cannot be what time means at all; and that McTaggart's substitution will not hold. For if being in time means being an imperfect representation of eternity, the statement that eternity appears in its perfection at the end of time can mean only that it appears perfect when it does not appear imperfect. Even this statement introduces a time that cannot be the time which McTaggart equates to the series of representations; in any case, McTaggart's whole system is significant only if time is something more than the series of representations. And this involves the admission that time is real, or at all events that its "appearance" has not been accounted for.

The second contradiction referred to—that of treating eternity as in time, and as appearing only at the end of time—appears as the argument develops. McTaggart contends that

it is intelligible to say that eternity is future or past but not (except metaphorically) that it is present. "If it were present, it would bear the [same?] relation to our present position in the time series as the present does—that is, of course, it would have to be identical with it. And the timeless reality is certainly not identical with a position like our present one, which represents it as in time, and, therefore, according to our present position, represents it inadequately." The argument appears to be that by "present" we mean a certain imperfect manifestation of the eternal. But since the eternal cannot be identical with that manifestation, it cannot be present. Yet it is the eternal, on McTaggart's own showing, that appears with any particular degree of adequacy; it can as well be present, therefore, as it can be future or past. It must, in fact, occupy every moment of apparent time. Apart from this point, the argument that the eternal cannot perfectly manifest itself in the present is devoid of all significance. If "present" means an imperfect manifestation of the eternal, it is another identity; if it means the perfect manifestation of eternity, it is a self contradiction. Now, how could McTaggart determine which of the two meanings is rightly to be given to the term? We could settle the question if we could recognise the character of being present apart from the special degree of adequacy the present exhibits. Or we might argue that being a perfect manifestation of eternity is not a temporal state at all. McTaggart seems to use both points, though it is the second that appears more particularly here. It would prove equally well that eternity could not perfectly appear in the past or the future. However, it is here that McTaggart's position collapses. For, as we saw, the statement that eternity appears perfectly in the future, or at the end of the future, escapes being an identity only if time means something more than a series of representations of the eternal. Yet, on the other hand, the fundamental assumption that time runs up to, and ceases at, eternity is necessarily based upon the view that time is nothing else than the series of more and more adequate appearances.

Thus, to be of any significance, McTaggart's position would mean that the time in which things appear is different from the time that is said to be a series of representations. If this is the case, McTaggart has shown no reason for believing that the time series will end at eternity. Again, the recognition of time as an independent series would remove the point of the assumption of a series of representations, because it is being temporal that constitutes an event an imperfect manifestation of eternity: thus McTaggart speaks of things as appearing "to be more or less in time". If things are, or appear, temporal, as well as representing the eternal, there is no reason why the eternal, even when it manifests itself perfectly, should not appear

temporal also, *i.e.*, why we should accept the reality of any eternal at all. Thus, in trying to show that time leads up to the eternal, McTaggart makes a defence of mysticism impossible. The orthodox mystic has merely to find a proof of time's non-existence. McTaggart has to do not only this, but also he has to show that time is sufficiently real to make it impossible for eternity ever to be present, though it is future or past.

I have dealt here only with the second of the two conditions of the establishing of a "cheerful view of things"—the doctrine that eternity is future. In the preceding essay, McTaggart attempts to prove the unreality of time. Of course, when these two conclusions are rejected, there is not much of McTaggart's system that remains: it is obvious that a belief in the unreality of time could not but affect every other important philosophical issue. For his own part, McTaggart seemed to regard the question of the relation of time to eternity as the crucial issue for him; he says that "the determination of the relation of time to eternity is at present the most pressing and important question in philosophy". I should say that McTaggart was no more successful than anyone else has been in making mysticism theoretically presentable. Perhaps it is also true that no one ever made a more skilful attempt.

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